

THE MEDICAL REPOSITORY.

VOL. I.—No. IV.

CONTENTS.


	Page.		Page.
Mineralogical Sketch of New-York continued	445	Priestley on Phlogiston, P. II.	541
On the Bilious Fever and Dysentery at Sheffield, in 1796	453	Woodhouse's Young Chemist's Pocket Companion	542
Medical Essays, No. II.—On the Grenada Fever of 1793 & 1794	471	Huger on Gangrene and Mortification	543
On the Fever at Jamaica, in 1793, 1794, and 1795	496	Logan on Rotation of Crops	545
On the Mercurial Practice in New-England	500	Bordley on the same	546
On the Fever at Newbury-Port in 1796	504	Browne on Yellow Fever	547
Singular Cases of Disease in Infancy	507	Cooper on Stramonium	550
A Case of the same	511	Stock on Cold	552
On the Chemical Disputes relative to Phlogiston	514	Johnson on Fixed Air	553
Dr. Priestley's Reply	521	Proceedings of the College of Physicians of Philadelphia	556
On the Epidemics of BETHLEEM, Connecticut	523	Walker on Sterility	ibid
On the Climate of the North-Western Lakes, with Thermometrical Observations	526	Meteorological Observations	557
REVIEW.		Table of Hospital Patients	560
Yates and Maclean's View of the Science of Life	531	Return of Dispensary Patients	564
		MEDICAL NEWS.	
		Domestic	567
		Foreign	573
		Correspondence	582

NEW-YORK:

Printed by T. & J. SWORDS, Printers to the Faculty of Physic of Columbia College, No. 99 Pearl-street.

1798.

THE
MEDICAL REPOSITORY

 *The Copy-Right of each Number of the MEDICAL REPOSITORY is regularly secured according to law.*

*In
N
an
ed
to
fo
su
de
de
vo
me
th
m*

ADVERTISEMENT.

IT was the design of the Editors to give, with the present Number of the REPOSITORY, a general Title-page, Index, and List of Subscribers. Two circumstances have prevented its execution at this time:—A disappointment in respect to paper, which delayed the printing till there was only time for completing this number, and the non-reception of the subscription-papers from several parts of the country. The delivery, therefore, of the Title-page, &c. is necessarily deferred till the publication of the first number of the second volume, which they will certainly accompany; and, in the mean time, the Editors once more request that the names of those Subscribers, which hitherto have not been sent to them, may be forwarded.

New-York, May 1, 1798.

ERRATA.

Page 460, line 13 from the bottom, for "more" read *mere*.

Page 496 is erroneously marked 471.

MEDICAL REPOSITORY.

VOL. I.—No. IV.

ARTICLE I.

A SKETCH OF THE MINERALOGICAL HISTORY OF THE STATE OF NEW-YORK.

By SAMUEL L. MITCHILL.

[Continued from p. 314.]

5. *The Alluvial Tracts.*

BY the term *alluvial*, is understood, not merely the substances brought down to the vallies, by the wash of rivulets and rains, and the crumbling to dust of the hills and mountains, but the matters formed from the growth of plants, in low, swampy, and intervale spaces, adjacent to lakes and rivers.

And, first, of the vegetable matter collected in wet and marshy places. This is frequently of an inflammable nature, and answers very well for fuel. When used for burning, it is called peat or turf. If employed in fertilizing the ground, it is termed swamp manure. The true history of this substance has not been generally well understood, for it has been commonly classed with *mineral* substances, though Mr. Kirwan, who retains it in his mineralogy, (*Elements, &c.* Part iii. sp. xi. and xii.) has removed it from the class of *earths*, where it had been placed before, to the class of *inflammables*. "There are," says he, "two sorts of inflammable substances, known by the name of 'peat.' The first and principal is of a brown or black colour, found in a moory ground, and when fresh, of a viscid consistence, but hardens, by exposure to the air. It consists of clay, mixed with calcareous earth and pyrites, and sometimes contains common salt. While soft, it is formed into oblong pieces, and the pyritaceous and stony matters are separated. When distilled, it affords water, acid, oil, and volatile alkali, and its ashes contain a small proportion of fixed alkali. They are either white or red, according as it contains more or less ochre or pyrites. It is found in Scotland, Holland, and Germany. Another sort is found near Newberry, in Berkshire. It contains but little earth, but

Vol. I. No. 4. B

"consists chiefly of wood-branches, twigs, roots of trees, with leaves, grass, straw, and weeds." "*Turf*," continues he, "consists of mould, interwoven with the roots of vegetables. When these roots are of the bulbous kind, or, in large proportion, they form the looser and worst kind of turf; but when mixed with a considerable proportion of peat, form what is called *Stone-turf*. It at first hardens, but at last crumbles, by long exposure to the air." The compilers of the *Encyclopædia Britannica* (page 1896.) have inserted the following remarks on this substance: "There are very considerable varieties of *peat*, proceeding probably from the admixture of different minerals; for the substance of peat is plainly of vegetable origin. Whence it is found to answer for smelting of ores, and the reduction of metallic calces, nearly in the same manner as the coals of wood, &c."

An accidental occurrence satisfied the mind of the Commissioner, as to the particular plant entering largely into the constitution of peat-bogs. Some time in the year 1786, as he was travelling in the western parts of Scotland, after his return from the Highlands, in company with two of his friends, they directed their walk on foot from Greenock, across the country to Paisley. During this excursion, they crossed an extensive bog of peat or turf, not far to the westward of this town; and there remarked for the first time, that the uppermost stratum of peat consisted of small plants, which, though dead, were in a state of intire preservation. Their particular shape, their genus and species, could be plainly distinguished. The principal among them was the *sphagnum palustre*, which, upon examination, constituted almost the whole of that deep and extensive bog. On examining beds of turf in America, the same observation has been verified by numerous observations of these plants, in their living as well as decayed state. He thinks himself warranted in concluding the basis of peat and turf to be the sort of moss called, by Linnaeus, *sphagnum palustre*, (Sp. Plantar. p. 1570.) "which," that author says, "grows in the deep bogs and fens of Europe."

This plant delights to grow in wet, low, or swampy places, and requires for its support a plentiful supply of water. It will also thrive in elevated situations, if sufficiently moistened. For, on high mountains, where water is so plentifully condensed as to preserve constant wetness, the *sphagnum* grows vigorously. When a plantation of these vegetables is formed on a low-land bottom, the parent plants, after producing and maturing their seeds, die, and form a soil for their offspring to grow upon; for as it has been long ago ascertained, that some vegetables can grow without being rooted in earth, it is here found, that *dead moss* answers all the purposes of a soil for the new generation of moss to grow upon.

After a while, this crop of plants dies, and adds to the increase of the vegetable matter beneath; and thus, from year to year, the process goes on, the old plants decaying, and forming a soil for the seeds they left to sprout in; until, by degrees, a bog of moss be formed. And this happens, because the plant which had lived in water does not very speedily corrupt in it, but continues to accumulate layer upon layer. It hence follows, that the depth of the bog will be proportional to the number of successive decays of these swampy productions, and to the circumstances favourable to their thriftiness.

In its fresh and living state, it retains water like a sponge; and, on account of its being so retentive of moisture, it becomes a soil for aquatic plants of various sorts, which absorb nourishment from the water, and other matters inherent in the moss. These vegetables taking root, growing in the peat, and decaying there, give to it the mingled appearance of roots, leaves, stalks, branches, bark, trees, and other extraneous substances, so frequently blended with it.

When peat is left to itself, to grow in the natural way, it frequently rises many feet above the original surface of earth; and the stratum continues to enlarge and thicken, as long as there is water enough to keep the moss alive. When this fails, the plants die, and the process stops; for, after it is raised considerably above the level of the springs that feed it, the rain of the season is neither regular nor copious enough to supply the demand both of it and the plants rooted on it; they consequently perish. Therefore, the ditching of swamps and meadows where the sphagnum grows, will drain off its requisite supply of water, and quickly destroy it; and it is accordingly observed, that where such low lands are, by art, rendered fit for manuring, pasture, or tillage, the growth of peat is stopped, and a sward of grasses is formed above the inflammable matters, which is thereafter wholly concealed from sight, and is never to be seen until after the sod is dug away.

As the peat is thus formed, layer over layer, in the course of successive vegetations, it can be easily explained how trunks of trees, fossil wood, and bodies and bones of animals, came to be buried so deep below the present surface; because, at the same time, when the trees fell, and animals died, in the places where they are now found, they were upon the top, and, by the perpetual growth of the plants around, they have, in many places, become covered to a great depth.

If there is any difference between peat and turf, it consists in the degree of putrefaction, to which the former has been subjected more completely than the latter; and has lost, therefore more of a vegetable appearance, and become more completely divested of its organic nature. As the other vegetables growing in the

sphagnum, have each of them somewhat of peculiar qualities, it must follow, that *turf*, when chemically examined, will afford a greater difference of result, and this variety will be rendered yet greater by the mixture of earths, stones, and other mineral substances, which the winds and torrents have carried among it.

From all these particulars it will be apparent, that both turf and peat, when pure, ought to be considered merely as a residuum of decayed vegetables. That the minerals frequently found in it, are foreign and casual admixtures, by no means essential to its nature, and that it ought not to be considered as a mineral production, nor classed as such in the systems: but that the clay, marl, pyrites, and other fossil bodies found among it, should be referred to their proper places in the mineralogical arrangement.

On this subject, there appears a propriety in suggesting a few practical hints. As wood grows scarce, and our State is not known to abound in coal, the sphagnum might be cultivated for fuel in wet grounds and swamps that now lie waste, and perhaps will never be cultivated. If this should ever be attended to as an economical matter, the cultivator should not suffer cattle to tread it, nor foreign plants to overgrow it, nor water to be drained from it; but clear the swamp of the peat as soon as it has risen above the moistening influence of the springs, and be ever careful to remove the upper paring, containing the living plants, to some wet and suitable spot for propagation.

But though the sphagnum has so much agency in filling up wet and low places, it is by no means the only plant which acts in that way. There are many other small and obscure vegetables, which, by their numbers, add considerably to the bulk of matter accumulated in these spots. Many of the grasses taking root, and increasing upon such bottoms, form, in process of time, bogs, hassocks, or a sort of sward, which contribute no less to the diversity, than to the increase of these swampy productions. When shrubs and trees, of various kinds, as well as animal plants, gain an establishment in these soils, the qualities and appearance of the latter undergo additional variation.

In this way are many considerable portions of sand, furnished with a covering of vegetable matter, with very little foreign admixture. But a more common case is, that in the neighbourhood of low places, there are high grounds and mountains, from which there is a constant descent of the matters of which they consist to the vallies.

Whatever may have been the cause of the inequalities at present existing on the surface of the globe, observation teaches, that the hills and mountains are generally in a crumbling condition, and descending gradually to the plains below. The chief agents in this process, which is incessantly changing the face of things, are,

1. The undermining of waters. 2. The washing of rains and torrents; and, 3. The irresistible force of frost.

The undermining operation of the ocean water, is very evident on the south side of Montauk-point, at the east end, and on both sides of Cow-bay, on the north side of Long-Island, and in many other places which it would be easy to enumerate. Instances of the effect of streams and rivers, in altering the disposition of the solid materials through which they run, occur at Passaic Falls, above Second River, in New-Jersey, where huge masses of rocks are evidently displaced; at Kaat's-Kill, where part of a hill has fallen down; and in various places in the Blue Mountains, where, after the more soft, moveable substances that had lain below, have been carried off by the rains and floods, solid fragments of the materials that are deprived of their support, tumble down, and seek the level of the plains.

By the force of waters, descending from hills and mountains, the firm materials forming the channels and sides of the streams are incessantly wearing away. Sandy and moveable matters, washed by rains from the mountain tops, leave the more durable rocks behind, in all their rudeness and nakedness. Every shower that falls denudes more and more of the rocks, by carrying down constantly the loose materials that cover them. Each rill and brook, however small or insignificant, that trickles along, labours, with all the force it possesses, in the same work of alteration. Whatever of earthy, saline, metallic, or inflammable, the soil of the upland contains, is thus floated or rolled along to the low lands, and constitutes, with proportional diversity and mixture, the intermontane soil. The bars of arenaceous matter off Sandy-Hook, where the Hudson joins the Atlantic, and, at the disemboguing of most great rivers, are plain confirmations of this procedure; as are the shallows between the bays of Tappan and Haverstraw, and the Oversloughs, in the neighbourhood of Albany. The formation of deep and large gulleys on the side-hills of farms, the hardening of miry places, the filling up of ponds, the consolidation of sloughs and quagmires, are all of them confirmations of the reality and extent of these operations. Alluvial deposits of this nature will necessarily be as various as are the ingredients of soil which they wash. Accordingly, where clay, in large quantity, has been suspended in, and diffused through water, it has formed on its deposition vast strata, which underlay great tracts of level country. Thus, the flat, between the basaltic rocks and Stony-point, at Haverstraw, appears to be wholly underlaid by argillaceous strata, which, as their edges appear along the shore, are not yet hardened enough to withstand the impression of the walker's foot. Much of the level country between Red-Hook and Greenbush, is evidently bottomed on a stratum of clay. The tract between Albany and Sche-

nectady, has a stiff and thick layer of clay beneath. The same remark applies to the space, lying for some miles beyond the Mohawk River, toward the Ball-town Springs; and to the great collections of similar earthy matter, lying westward along the flat country, between the Upper Falls and Fort Stanwix. It would be easy to give other instances of extensive strata of argillaceous alluvia, but these appear to be sufficient.

When silicious sand is the prevailing matter which the floods bring down, then the soil will consist chiefly of this, as in all the instances just enumerated, where, since the argillaceous layers were deposited, a stratum of loose sand, of considerable thickness, has been strewed over their surface, and, in most places, completely covers it up.

Where clay and sand, instead of being deposited successively in separate layers, as in these instances, have subsided from their watery connection in mixture with each other, they form *loams* of different degrees of friability or stiffness, as the silicious or argillaceous ingredient happens to predominate. And this is by far the most common case that occurs; the mixture of clay and sand forming the rule; and the occurrence of them in their separate states being the exception.

If water issues from hills, consisting of calcareous earth, or abounding in lime-stone, it follows, from the known solubility of this substance in that menstruum, that a portion of it must, in such form, be conveyed to the country below. While, in addition to this, whatever portions of calcareous rock had been broken off, and reduced to a smallness sufficient to be moved by the currents, will be dragged down their channels, and eventually reach the plains. In these ways, the calcareous earth seems to have been deposited in some moory and swampy bottoms, in quantity enough to constitute, with the other matters it meets with, marles of various kinds and goodness.

In several spots along the shores of the Hudson and the Mohawk, the ferruginous sand which may be collected in considerable quantities, affords evidence enough of the abrasion of iron from the places where its ore abounds. This metal, in the form of oxyd, or ochre, or sulphate, thus joins itself to the other materials of alluvial strata; or, in the form of chalybeate water, imparts to them its tincture and impregnation. Bog-ore, (*Ferrum Tubal-Caine*) has evidently been deposited, and accumulated, where it now abounds, by the operation of similar causes, rather than, as some ingenious persons have imagined, from the decomposition of organic matter.

Another agent, in forming these secondary accidental strata, is the expansion of water, as it consolidates to ice. The crystals, into which it shoots, on its congelation, are so arranged in

respect to each other, that they are incapable of being contained in a space as small as the water occupied. On the withdrawing of heat to a certain degree, there necessarily follows an assumption of this crystallized figure by the water; and, consequently, an enlargement of its volume. The force which ice is capable of exercising, when compressed or confined in such circumstances, exceeds all calculation. Nothing appears either too heavy for it to lift, or too strong for it to break. The time when it is particularly active in splitting rocks, and tearing down the structure of mountains, is when, after falls of snow, there succeeds a day warm enough to melt it, or rain in sufficient quantity to fill the chinks and fissures. Whenever the vacuities are thus penetrated by water, and a freezing night, or continuance of cold weather succeeds, the hardened water being under necessity to occupy more space than before, rends the masses of rocks asunder. If the strata between which it has insinuated itself are horizontal, it elevates the upper ones; if perpendicular, it crowds them apart: if on the side of a precipice, the unsupported portion, on the instant of its separation, rushes down with impetuosity, to find a resting place below. Occurrences of this kind are frequent in our mountains, and among the basaltic rocks in particular. The noise of the falling fragments is often heard to a considerable distance. Nor does the operation of frost confine itself to the dislodgment of rocks in large portions. Wherever water can penetrate the open texture of freestone, or find its way among the bibulous or porous particles of calcareous and argillaceous rocks, there, during the season when melting heats are rapidly followed by hardening colds, the solid masses become reduced to a powdery consistence, and their surface crumbles away in the form of gravel, grit, or sand. The vestiges of this process are so frequent in the mountains, that as the Commissioner saw rock detaching itself from rock, and particle receding from particle, these scenes of decay and ruin impressed him, in the strongest manner, of the revolution which, even in his own time, and before his own eyes, the world is undergoing.

In giving an account of the substances which enter into the composition of these recent strata, and the sources whence they are derived, the tincture which the mineral and vegetable matters composing them, receive from the addition of animal substances, must not be omitted.

These moist and level spaces now described, are exceedingly well adapted to be the dwelling-places of animals; plenty of vegetables for them to feed on, and of water to live in, make many of these spots teem with creeping things. The speedy death and renovation of many tribes of these living creatures, in numberless swarms and multitudes, however small the individual may be,

give an idea in the aggregate of a vast sum of animal matter. Myriads of these, viewless and unknown, after having ceased to perform the functions of life, go into the common mass of extraneous substances. When they are of the testaceous kinds, their calcareous coverings, mingling with the other materials of the soil, and crumbling, by degrees, to dust, constitute shell-marl, (Humus Conchaceus.) Where large quantities of animal substances have decayed in any spot so frequently, as that their relics form the greater part of the upper stratum of earth, the product is animal-mould, (Humus Animalis.) If, in such situations, animals have died bemired, or in any other way, their ribs and thighs, discoverable by digging, several feet beneath the present level of the surface, as at the Wall-Kill, satisfy us of the reality of these accretions, but leave us oftentimes doubtful of the species of creature whose bones are under examination.

(To be continued.)



ARTICLE II.

An Account of the BILIOUS FEVER and DYSENTERY, which prevailed in Sheffield, Massachusetts, in the year 1796.

By WILLIAM BUEL, Physician.

IN a letter to Mr. Smith, published in Mr. Webster's Collection of papers on the subject of bilious fevers, I have given some account of the febrile disorders which prevailed in Sheffield, in the years 1793, 1794, and 1795.

In a country town, where the air is not rendered insalubrious by a great number of inhabitants being crouded into a small space, and where habits of indolence and luxury have not sown the seeds of disease, a sickness, affecting even so great a proportion of inhabitants as that which prevailed in those years, is a rare occurrence. But one so nearly universal, and attended with so great a mortality, as that which makes the subject of this communication, is perhaps unequalled in the annals of our country.

To describe the history of the sickness which prevailed in the year 1796, with as much correctness as the importance of the subject deserves, it would be requisite to premise a minute and circumstantial topographical description of that portion of country where it appeared; a correct account of the disorders which had been prevalent in the preceding years; and results of meteorological and other observations relative to the air and weather, accurately made for each of the preceding sickly years; materials for which I have to regret that I am not in possession of.

The necessity for these things becomes, however, in some measure removed, when we recollect that the principal cause of the sickness, which has so severely afflicted the people of a part of Sheffield, for several years past, admits of the most conclusive demonstration. There are few points in Medicine, more firmly established than that of the mischievous effects of marsh effluvia.

For an account of the sickness of the preceding years, I shall refer to my letter published in Mr. Webster's Collection, and shall attempt

A sketch of the situation of that part of Sheffield, which was the scene of sickness in 1796, with a few remarks relative to the weather of that year.

The part of the town in which the sickness prevailed, is almost a perfect level. The river Housatonak, whose width is generally between thirty and forty yards, runs through it in a ser-

pentine direction, and with a very gentle current. Its banks at low water, are from eight to fifteen feet high; the bottom, for the most part, is soft and muddy; and its depth such as to make it fordable in but a few places. On each side of this river, there is a considerable extent of luxuriant meadow ground, whose surface is generally overflowed when the snow melts in the spring, and sometimes by freshets, at other times in the year. This meadow ground is all much interspersed with coves or pools, which are left, after the subsiding of the flood, full of stagnant water. The water contained in these coves, which are, in fact, great reservoirs of animal and vegetable filth, is, in the course of the summer, evaporated from some to dryness, from others nearly so, and from all in a degree proportioned to the dryness of the summer.

Beside the meadows adjoining the river Housatonak, there are several other streams which run through large tracts of flat and very marshy land. On one of these streams towards the north part of the town, is the mill-pond which appeared to be the common center of the sickness in 1796, and the preceding sickly years. This pond overflows a large tract of land which was formerly covered with a luxuriant growth of timber, and other vegetable productions, and which are all now dead and in a state of dissolution, in consequence of the action of the water upon them.* Whenever a dry season occurs, the water recedes from almost the whole of the land last flowed, and leaves the whole mass of dead animal and vegetable substances lying on its surface, exposed to the action of a scorching sun.

From a view of this state of things, it is not unnatural or unphilosophical, to expect the commencement of a putrefactive process upon an immensely large scale. That this effect does in fact take place, is demonstrable, not only by our reasoning faculty, but by the testimony of our senses. The fætor which arises from the surface of this drowned land, when made bare by dry and hot weather, is extremely disagreeable and offensive to all who approach its borders. It is strongly perceptible to travellers who pass on the roads within its vicinity. The stench is smelled by the inhabitants, at times, even to the distance of half a mile. An exposure to the effects of this noxious effluvium, contiguous to its source, not unfrequently, in the year 1796, produced immediate nausea and vomiting. A man who spent a few days at the house

* It is important that it should be remarked in this place, that this pond, although it was originally raised about the time of the first settlement of the town, which is between sixty and seventy years ago, was, when the dam was rebuilt, about twelve years since, raised about seven feet perpendicular, by which means the water spread over a much greater extent, of surface, than it had done antecedently to that time. It is on the land last flowed, that the substances, to which I allude, exist.

of a relation of his residing near the pond, told me, that while traversing the banks of the pond in pursuit of game, he experienced a stench so excessively offensive, that it immediately produced nausea and vomiting, which he was not entirely free from, until he was attacked with bilious fever, at his own house, in a very healthy part of the town.

I will, as well as I am able from memory, make a few remarks, relative to the spring and summer of 1796, which, perhaps, may not be totally unconnected with the sickness of that year.

In the latter part of spring, when, in common years, we have mild and pleasant weather, suited to bring forward the productions of the earth, we this year had, almost uniformly, cold, cloudy, and rainy weather. The spring of course was very backward. We had such an excess of rain even through the month of June, that all our streams, ponds, coves, and marshes, were kept very full, and even our dryest land was highly surcharged with water. But from the beginning of July forward, we began to suffer from the other extreme: we very seldom had rain, and uniformly the weather was intensely hot, particularly in the month of August. In all the latter part of summer, and beginning of autumn, the earth was not refreshed by rain, nor the air by thunder and lightning. Indeed, the oldest person living does not, perhaps, remember a summer in which there was so little thunder and lightning. The drowth was so great, that vegetation was much injured; grazing grounds particularly were parched almost to perfect dryness. The springs and streams were so much dried up, in many places, that it was with great difficulty that water could be procured for cattle.

In the month of September, we were almost every night enveloped in thickest fogs.* Such abundant evaporation of water rendered the nights and mornings, until the sun had dispersed the fogs, most intensely cold; while, at mid-day, we were scorched with an intolerably hot sun.

Mosquitoes, which are always remarkably thick about the stagnant waters of this town, were this year, it was remarked, much more numerous than usual.

About the 20th of September, when the sickness was at its height, the weather suddenly grew cold and serene, and continued

* It is not probable that the cause of the pestilence which we experienced invaded in the form of fog. The poison of marsh effluvia is, probably, too subtle for ocular detection. Fogs are, perhaps, no indication of the unhealthiness of a place: they as frequently proceed from pure and running water, as from that which is stagnant and corrupt. It is probable, however, that such alternation of the extremes of heat and cold, as the body is exposed to from great fogs, in hot weather, increases its susceptibility of being operated upon by the causes of disease.

such for two or three days. Great expectations were entertained, that an abatement of the sickness would be the consequence of this alteration; but no such abatement took place; people continued to be attacked as frequently as before, until about the end of the month.

How much, or how little, all of the circumstances which I have related, affected the succeeding sickness, I shall not in this place hazard an opinion. One inference, however, seems naturally to result from a consideration of them; the keeping up of the water to an unusual height, over a large extent of surface, covered with putrefying substances, and the then sudden withdrawing of the water, and the consequent exposure of them to the action of a hot sun, are circumstances extremely well adapted to produce and give activity to great quantities of that pestilential vapour, which is called marsh effluvia. In confirmation whereof, I shall attempt

A particular history of the sickness of 1796.

On the evening of the 6th of July, I was requested to visit two daughters, one about eight, the other about six years old, of a Mr. Fairchild, who lived about half a mile, in a south-eastern direction from the pond. I was informed that they had been taken the day before, with a looseness, which had increased to such a degree, that the parents thought it necessary that it should be restrained. I did not at first suspect the disorder to be dysentery. I had never before known an instance of that disease in the course of my practice in Sheffield, which had been several years; what I directed was, of course, not with a view to that disorder. The next day I was convinced that their complaint was a genuine dysentery. A day or two after the sickening of the children, Mrs. Fairchild was attacked with symptoms similar to theirs, but so slightly, that she was able to attend to her children for two or three days afterwards. The eldest child, who had been infirm, died in about four days from her attack. The mother and the other child recovered, but not till after they had suffered severely from the disorder. Of three or four persons who belonged to the family, none were in any manner affected with the disorder. Nor were any of the nurses or attendants on Mrs. Fairchild's family, or any others who had communication with them, attacked sufficiently early to render their infection ascribable to contagion from them.

About the 10th of the month, another child, at seventy or eighty rods distance from Mr. Fairchild's, and one at the distance of two miles, were taken with the dysentery. About the 17th or 18th, a girl, and soon afterwards several others in the family, at about three fourths of a mile distant from Mr. Fairchild's, and within a few

rods of the pond, were taken. By the 20th, there were a number of other scattering instances, within the sickly circle which I shall hereafter describe; and by the close of the month, there had been about twenty persons, the most of them children, attacked, and several had died.

About the 20th, three youths of one family, were taken at nearly the same time, with febrile symptoms, which, although disguised at first by some peculiarities, turned out to be bilious fever. From this time, instances of this fever frequently occurred, so that it was apparent both disorders were endemic, and becoming frequent. In a short time, both prevailed to a degree truly calamitous and alarming.

The following description, together with a statement of the number of deaths, will convey some idea of the distressed situation of the people who dwelt within the limits of this ravaging sickness. Let an imaginary circular line be described, from a point on the south-eastern side of the above-mentioned mill-pond, whose radii shall be one and one half mile in length. This circle will embrace about one hundred families, and about six hundred inhabitants. It would comprehend the whole territory, in which the sickness prevailed, with so much exactness, that there would be considerably short of ten families without its limits, in which there was sickness; there certainly were not ten within which were exempt. Of this number of inhabitants, at least half were, in the course of the season, affected with either bilious fever, or dysentery.* The eastern half of this imaginary circle was the most populous; it was also the most sickly. Out of about four hundred and fifty persons which it contained, at least two hundred and fifty were affected with sickness. Of the one hundred and fifty who dwelt nearest the pond, there were not ten who escaped. Of the remaining one hundred and fifty who inhabited the western semi-circle, there were about fifty affected.

To the people who dwelt within this small description of territory, this distressing sickness was most distinctly confined. They who before this time had been in the habit of knowing but a few deaths among them in the course of a year, now hardly knew a day to pass without witnessing the solemnity of a funeral, sometimes of two or three in a day. Many whole families were all

* The eastern part of this circle would extend across the Houfatonak river, and would embrace about one hundred persons, who dwelt on the eastern side. It may become a question, admitting that marsh effluvia were the sole cause of the disorders in question, how far their sickness was ascribable to the stagnant water in and about the river, and to which they were contiguous, or to the distant and more productive source of the poison, the pond. However this may be, the proportion of sick was much less in this part of the circle, than in that about the pond.

down at a time, and some of them in the agonies of death; sometimes without being able to procure a single person to perform the office of nurse or attendant. Even after the closing scene had past, it was with difficulty that help could be obtained to perform the last sad offices of humanity.

During the prevalence of the bilious fever and dysentery this year, there were no less than forty-four persons, from the inhabitants of the above described circle, who fell a sacrifice to one of those disorders.* Of this number, twelve were adults, and thirty-two children. The children, all except two, had dysentery; the death of one of those two, whose disorder was originally bilious fever, was caused by an abscess; the other had convulsion fits. Among the fatal cases of adults, there was but one male. He was a man of between fifty and sixty years of age; and had both bilious fever and dysentery. He neglected to procure any medical aid, until he had been affected with the dysentery a week, and with bilious fever a fortnight. Even then, his first application was to a quack, whose nostrums, he had the candour to confess in his last moments, he believed injured him. After this period, his disorders resisted the usual remedies.

Three of the adult females were women in a state of preg-

* Since writing this account, I find two or three of the deaths were of children who belonged contiguous to a mill-pond, at the south part of the town, and who died of dysentery. There were a few other instances of dysentery this year, as there had been of bilious fever for several years past, at the same place.

In the letter to Mr. Smith (referred to in the beginning of this paper), it is remarked, that the Fever which prevailed in Sheffield, in 1794, was principally observable in the immediate vicinity of the *South Pond*, while that which prevailed in 1795, as is usually the case, was in the neighbourhood of the *North Pond*; and that, at that time, I was unable to learn the cause of this diversity in the theater of the disease, in those two years. This I have since discovered, and deem important enough to deserve to be recorded.

It appears, on investigation, that, in the year 1794, the waters of the *South Pond* were drawn off, for the purpose of repairing the dam, or the mills. This left a large extent of putrescible materials bare, and the progress of the season gradually produced those morbid exhalations which I suppose to be the cause of Fever.—Indeed, this fact sufficiently explains why, *ceteris paribus*, that part of the town should be more sickly in 1794 than in common years.

Again, I am persuaded, from my own observation and the uniform testimony of others, that the land flowed by the *South Pond*, is kept much more constantly and completely covered with water than that flowed by the *North Pond*. The importance of this distinction will be obvious to every mind; and will, probably, be thought to go far towards accounting for the difference of insalubrity in the atmosphere of the two ponds, in common years, and under ordinary circumstances.

nancy, and who died of bilious fever. Of the remaining eight, one was a very aged woman, two laboured under chronic disorders, by which they were greatly debilitated; they died of dysentery. Three of the remaining five died of dysentery complicated with fever, and two with bilious fever alone. Hence it will appear, that out of this whole number of mortal cases, there were but two from bilious fever alone.

Symptoms of bilious fever and dysentery, with some remarks on the prognostic in each disorder.

The symptoms of attack in bilious fever, differ very little, except in degree, from those in common ague and fever. The patient sometimes has pains and rigors running over him, for several hours, before he has a proper fit of ague; but generally a chilliness, which soon increases to a violent fit of cold and shaking, is one of the first symptoms. It is accompanied with hard pain in the head, back, and limbs, particularly in the back. The pulse in the cold fit, is low, quick, and hard. Towards the close of the cold fit, nausea, and not unusually a bilious vomiting, occur. The length of this fit is uncertain; it lasts from one to a number of hours. At length the rigors and shaking begin to subside, and the patient becomes sensible of great heat and thirst. The pulse becomes full, frequent, and hard, and sometimes rebounding. The face is flushed with redness; the eyes assume a red, watery, and inflamed appearance. This appearance of the eyes; is usually proportionate to the violence of attack, and the future force of the disorder.

The duration of the hot fit is extremely various, it lasts twelve, twenty, thirty-six, and sometimes forty-eight hours; that is, the fever, in some instances, particularly at the first attack, runs on for that length of time, without suffering any abatement that may be called an intermission, or even remission. It is true, nature, in the course of the time, will sometimes seem to make efforts to produce a termination of the paroxysm, by salutary sweating; a moisture will just begin to appear, but will be suddenly dissipated, and the fever will kindle up with renewed violence. When an effectual and salutary sweating does take place, it is commonly very profuse, and the more free and profuse, the more perfect is the relief obtained. The febrile symptoms now gradually abate, and subside either partially, so as to produce a remission, or, more perfectly, into an intermission.

The interval, too, which succeeds, is of uncertain duration. The great irregularity attending the duration of the different stages of the paroxysms, produce, in different cases, all the varieties of quotidian, tertian, double tertian, quartan, and even some which

it was difficult to reduce to either of the usual distinctions of intermittent fever. A regular tertian appeared to be the most mild and eligible form of the disorder, and that in which it was the most easily managable. When the fever was quotidian or double tertian, the intermission or remission was too short to afford an opportunity of doing much with the most important curative remedies. A regular quartan form was not frequent, although there were some instances of it.

The symptoms of disturbance in the alimentary canal, are a very important article in the description of this fever. I have mentioned before, that nausea and bilious vomiting were occurrences which frequently took place towards the end of the cold fit. Indeed, the presence of a preternatural secretion of bile is a never-failing concomitant, from the first attack of the disorder, to the termination of a lingering and tedious convalescence. Symptoms of flatulency are almost constantly attendant, particularly about the time of the approach of the sweating fit. Flatulence gives rise to great pain and distention of the stomach, belchings, and a sense of suffocation so intolerable as apparently to threaten an immediate extinction of respiration. The breaking forth of a freely flowing sweat commonly produces relief.

The appearance of the tongue in this disorder affords a very just indication of its degree of violence. In a mild degree of it the tongue is covered with a whitish fur, which is always moist. When the degree of disorder is somewhat more considerable, the tongue towards its point, especially at the height of the paroxysm, loses its moisture, and the dryness becomes extended as the degree of disorder is increased. In a still higher degree of violence, a black stripe appears, extending from the root to the extremity of the tongue. In the worst cases the tongue was covered all over with a very thick black fur, and remained at all times intensely dry and parched. In some of the very worst cases, it would be perfectly free from fur, and as dry as a husk. In the most violent case of this fever which I ever saw, although, from more strength and firmness of stamina, it terminated favourably, the tongue was from the beginning entirely free from fur. It all the while had a red and shining aspect, and was parched, or rather, as one would judge from its appearance, was roasted, with a husky dryness. The prognostic from a very black and dry tongue, was not that there was very great danger, but, on the contrary, that state of the disorder in which this symptom occurred, was almost certainly manageable by proper remedies. One of the first signs of amendment was the appearance of a moisture, and the "cherry-coloured redness of health," in a very narrow streak upon the edge of the tongue. This sign was hardly ever deceptive.

In a fever producing such general derangements of the system,

it was not to be expected, that the intellectual functions would escape unaffected. In almost all considerable cases, there was, at the height of the paroxysm, some degree of delirium, coma, or stupor. It was not uncommon for a stupid, comatose state to be constantly present for several days; sometimes to such a degree, that it was difficult or impossible to arouse the person. An imperfect state of the senses, particularly of the sense of hearing, sometimes took place, and continued, in some degree, through the convalescent stage of the disorder.

So remarkably protracted was the convalescent stage of this disease, that the symptoms thereof require some consideration. It has been supposed, that the frequency of relapse in this fever, and in common intermittent fever, is occasioned by some incidental extraneous causes, such as fatigue, errors in diet, exposure to cold, to night air, and the like: these circumstances undoubtedly may act as exciting causes to relapse; but I am convinced that the most rigid caution in these respects, will not alone secure persons who have had these disorders, from relapse. It is evident, that there is something in the nature of the contagion producing these fevers, which disposes it to operate on the human body, if I may so express myself, with an intermittent action. By intermittent, I do not mean to allude to the type of the fever, but the periods of the different attacks. The system, after it has become habituated to the contagion, and ceases to be operated upon by it for a time, at the end of a certain period loses that habit, and is again excited into morbid actions. That this tendency of the disorder is owing to its nature, and not to particular remedies made use of to cut it short, as has been supposed by some, is evident from the fact that it takes place, as well when the disorder is suffered to run its course, as when it has been shortened by remedies. This fact is ascertained by a very great number of instances of each kind, both in bilious fever, and common intermittent, which have come under my own observation, and to which, with a view to the establishment of this fact, I have given the most particular attention. As a confirmation of this idea of the disorder, I have found that the returns have always inclined to be periodical, and even when every precaution has been made use of, the relapses have returned at regular periods. And, however inexplicable, or even fanciful, the idea may appear to some, it is certain, that those periods are, in some way, affected by a lunar influence. In a great number of instances, repeated relapses have taken place, once in a fortnight, without varying in any instance more than one or two days, from the new, or full of the moon. In others, the intervals would be three weeks. But in all where I have observed, there has appeared a tendency to septenary periods. I have myself had four successive slight attacks of an intermittent, at almost exact intervals of

three weeks. I have known persons to be attacked regularly in periods of two and three weeks, with relapses of bilious fever, for three, six, and even twelve months, without interruption.

The commencement of winter appears to have some degree of effect in stopping the tendency to relapse; but its operation is not universal. So strong is the tendency of this contagion, to have a long continued operation upon the system, that it requires many months, and, in some instances, even years, to eradicate it. I know several gentlemen who contracted the disorder from the marshes in the western part of the State of New-York, more than two years since, who yet experience a continuation thereof in occasional febrile attacks.

The symptoms of attack in a relapse, are similar to those of the original disorder, but generally less violent; the intermissions are more perfect, and the disorder, in every respect, approaches nearer to the form and degree of common intermittent fever. A relapse generally terminates in the course of a few days of itself. A bilious vomiting and diarrhœa, seem sometimes to become a crisis to the attack. These symptoms, together with head-ach, appear sometimes to constitute the disorder, in relapse; fever being almost entirely absent. Indeed, it is peculiarly apparent, in the convalescent stage of the disorder, that the biliary secretion is eminently affected by the contagious cause. The face, and indeed the whole skin, is of a pale, yellow, and bilious hue. The urine, when evacuated, appears to be highly surcharged with bile. There are, at all times, evident indications of a preternatural quantity of this fluid in the stomach and intestines.

The usual precursor in DYSENTERY, was a griping pain in the lower part of the abdomen. This was, usually, soon followed by a continual tenesmus, and some small and painful discharges by stool. It was not common for those who were attacked with dysentery, to experience chills at the beginning of the disease, or to be immediately affected with much fever. Children, who frequently fell a sacrifice to the disorder in the course of three or four days, would, for several hours after they were attacked, continue to run about and make no great complaint; and adults would frequently continue about their business for several days after they had frequent dysenteric stools.

As both the diagnostic and prognostic in dysentery are chiefly to be obtained from the state of the stools, I shall endeavour to be very particular in my description of the various appearances which they exhibited.

The most common, and what might be called the natural dysenteric stool, was a small, whitish mucous discharge, mixed with streaks of fresh looking blood. In some cases, the blood was entirely wanting; this might be considered as a variety in the natural

form of the discharge. There was seldom any excrementitious matter mixed with these discharges, unless cathartic medicines had been employed; and they were nearly or quite free from any excrementitious smell; the smell of them was peculiar; but, in the beginning of the disorder, not very offensive. This state of the stools, when the disease terminated in health, generally continued from beginning to end. As it began to abate, the natural *faeces* appeared with the mucus, and the proportion of mucus became gradually less, until it disappeared, and the stools became natural. Among the deviations from what I consider the natural form of the dysenteric stool, was

A stool, in which the blood was diffused among the mucus, tinging the whole with a uniformly bloody appearance. This stool always indicated an unfavourable state of the disease. But when the stools were very liquid, and tinged with blood, resembling water, mixed with a small quantity of fresh blood, the indication was still more threatening. When the stools were of this last kind, I believe they were always involuntary, and that a fatal event ensued.

A stool, resembling beef brine, sometimes occurred; this briny stool was, in some cases, of a liquid, in others, of a mucous form; either kind was unfavourable, but not uniformly fatal.

I recollect the instance of a stool, in a child of five or six years of age, which was, in the beginning, pure mucus, free from any streaks of blood, and which gradually assumed a purulent appearance, until it looked like perfect pus. I supposed, in this case, that death was inevitable, but, to my surprise, the child recovered.

A greenness sometimes appeared in the mucus; it was indicative of an obstinate disorder; but when it appeared with the feculent matter, in the convalescent stage, the sign was not so unfavourable.

In the progress of the disease, an acceleration of pulse took place, and the patient was affected with considerable thirst. The tongue was covered with a moist white fur; and in the last stage of the disorder, it sometimes became dry and black.

The rapid emaciation of body, and exhaustion of strength, which take place in this disorder, are, perhaps, unequalled in any other. Many children sunk under it in the course of two, three, and four days; some who were able to walk about the house in the morning, would be dead at night.

The danger in the dysentery was proportioned to the frequency of the stools, and to the deviation of them from the natural dysenteric state.

The indications of approaching death, are a sinking or a total absence of the pulse in the wrist; a coldness of the extremities; insensibility to objects; a sinking in of the eyes; the eye-lids but partially closed, and the eyes so rolled up as to discover only the

whites of them; stools growing excessively foetid, and becoming involuntary; a hicoughing; tossing about in the bed; making now and then a forcible expiration resembling sighing: this last symptom is peculiar to children, and is an infallible indication of a fatal catastrophe. It was not uncommon in children, for convulsions to close the scene.

The circumstances here enumerated, many of them, perhaps, indicate nothing more than an extreme degree of debility; but they indicate a degree of debility, which, in this disorder, is seldom recovered from.

The dysentery frequently came on while the patient was affected with bilious fever. In this case, the type of the fever soon became obliterated, and the accompanying febrile symptoms were similar to those in original dysentery. The change of the fever into dysentery did not, however, secure the patient from the tendency to relapse so peculiar to that disorder. But the convalescence of those who had simple dysentery only, was generally short, and the recovery perfect.

Sometimes the fever came on upon the dysentery. The type of the fever was not, in this case, easily ascertained, until an abatement of the dysentery took place; when, as the dysenteric symptoms subsided, the fever would appear in its proper form. The two disorders appeared to be complicated, that is, they both seemed to exist at the same time, rather than to act in alternation. The fact is certain, that, in cases of accession of dysentery upon the fever, the latter disorder always shewed itself in its true form after the symptoms of the other had subsided.

Remarks on the cause of bilious fever and dysentery, and a consideration of the question, whether they were infectious?

That the stagnant waters in Sheffield, and the sickness which prevailed there in 1796, and the other late sickly years, stand in the relation of cause and effect, is, I think, a position which no person capable of reasoning, can withhold his assent to, after admitting, and candidly considering the facts which I have stated. I am sensible that new facts were not wanting to confirm a belief among physicians and philosophers; that marsh exhalations are a poison which most infallibly produces what are called bilious fevers. But, however astonishing it may appear, it is a fact, that many of the people who dwell in the vicinity of the stagnant waters of this town, and even those who have been the greatest sufferers in the several sickly years, disbelieve the local origin of their misfortunes, and strongly oppose all attempts to remove or lessen the force of their cause.

The history of facts, in our country, relative to dysentery, does

not seem to warrant the conclusion, that that disorder originates in all cases from the same cause with bilious fever. But the reverse of that conclusion seems probable, from the fact, that dysentery has frequently prevailed as an epidemic in places where bilious fever was never known, and under circumstances where we should never expect the latter disorder. Admitting the fact, that bilious fevers universally owe their origin to a local cause, that disorder can, of course, be only endemic, or confined to a particular place; whereas dysentery is many times most extensively epidemic.

But in the sickness which makes the subject of this communication, there is every reason to ascribe identity of cause to the two disorders. They were circumscribed in a very striking manner, by precisely the same limits; they both began, and ceased to prevail at the same time; neither disorder occurred (except in a few instances of both disorders about the pond at the south part of the town), at any considerable distance from the limits, but in persons who had previously resided within them. There were instances of both disorders, affecting persons in different parts of the country, who *had* resided within those limits; a stay of only one night in the central part of the sickly territory, in some instances, produced the disorders.

The facts which I have stated prove sufficiently, that neither of the disorders were propagated by specific contagion, at least, beyond certain boundaries; otherwise they must have extended, for there was no interruption of communication. I have remarked before, that I was myself convinced, that neither disease was propagated by specific contagion, even within those boundaries. In all the cases, which came under my observation, of sickness without the limits, and acquired by a residence within them, there was no instance of either complaint being communicated from the person affected. In several cases of persons who acquired their disorder here, and sickened at a distance, I find, from authentic information, that the fact is the same. I have, as an exception, to mention the information which I have received from Doctor Orton, of Woodbury, in the state of Connecticut. The Doctor relates, that a young man of this town, who, in the time of the sickness here, went to Woodbury, was, soon after his arrival there, taken down with dysentery, and in a few days died. That in about six days after his death, a person in a neighbouring house was taken with the same disorder; and that, soon afterwards, all the children, who remained in the vicinity, all the nurses, one excepted, and many others, sickened with it, and that nine persons in the whole fell a sacrifice to it. He further adds, that at the time the young man was taken sick, the town was free from any instance of dysentery, or any prevailing fever. From this statement it results, that the dysentery was, in Woodbury,

propagated by specific contagion, from one person to another, and that the contagion was extremely active. How can these facts be reconciled to those which I have related as taking place in Sheffield? I have remarked already, that I was convinced myself, that neither the bilious fever nor dysentery was propagated by specific contagion, even within the sickly territory. As there appears to be a clashing of facts in this case, I will particularize some of the circumstances upon which I founded my belief. The disorder, as I have repeatedly mentioned, was certainly circumscribed by exact limits; admitting it to be infectious, it could not have been for the want of communication that it was not extended, for the parts contiguous were on all sides populated and intercourse between the affected and well, was not attempted to be interrupted. At the commencement of the sickness, persons were attacked at about the same time, at so great a distance from each other, as to remove all suspicion of their taking the disorder one from another. There were many families where one person only had the dysentery, and all the rest escaped. Those who were the most exposed to the action of the effluvia arising from the sick and from their stools, escaped infection; while those who were free from any such exposure, were attacked: I was myself, many hours in the day, for more than eighty days in succession, in situations to have my nostrils assailed by effluvia from the worst of dysenteric stools, but I escaped the disorder. Other physicians, and several nurses, were almost equally exposed, and without effect. Had the dysentery been infectious, within certain limits in Sheffield, I should have supposed a predisposition acquired from some local cause, to be necessary to give the contagion action, and I should have considered the people of Woodbury under the operation of the same predisposition: this, considering the flatness of the land in Woodbury, and the peculiarities of the summer and autumn of 1796, would not be unphilosophical. But I will content myself with having stated the facts, and shall leave them unaccounted for.

Since I have had opportunities of observing the symptoms of dysentery, the subject of its proximate cause has arrested somewhat of my attention. At a time when new theories of diseases are almost daily presented to the world, I have met with no one of the proximate cause of dysentery, from which a solution of the phenomena of that disorder seems to result. Dr. Cullen's idea of a spasmodic constriction of the colon, seems better adapted to an explanation of colic than dysentery. A constriction, it is obvious, does take place, but is it not probable that this constriction is a symptom, and not the proximate cause of dysentery? Why is it not probable that the principle of contagion, whether derived from a person affected with the disorder, as it sometimes is, or from the air, as I suppose it was in the sickness under considera-

tion, acts as a stimulus upon the *intestina crassa*, producing an increased action of their excretories, or perhaps an inverted one of their absorbents? Is not this idea analogous to what takes place in small-pox, measles, lues, and many other diseases which seem to depend on the operation of contagion upon some appropriate part? As a confirmation of my idea, I observed that there was every appearance in those who had dysentery, that all the derangements which took place in the system, were the effects of this primary affection of the intestines. What are the chemical qualities of that principle, in a vitiated atmosphere, which constitutes contagion, or, the same thing, which produces these disorders, I leave to those philosophers to investigate, who, of late, appear to be cultivating this branch of science, with a zeal and success which promise great future benefit to medicine. But in the sickly season which I have been describing, I have abundant reason to believe that the principle, whatever it may be, which produced bilious fever and dysentery, was the same. In the dysentery, it operated upon the lower intestines; in bilious fever, upon the stomach, liver, and arterial system. The modern idea, that the affection of the liver in producing a preternatural secretion of bile is one of the effects of the contagion, and not the cause of the disorder, is most certainly well founded.

Cure of bilious fever and dysentery.

After much additional experience in the disorder, I find no reason to alter my opinion, which is published in Mr. Webster's Collection, respecting the cure of BILIOUS FEVER. It consists in the use of purges of calomel and jalap, in divided doses, in the beginning of the disorder, proportioned, in quantity and continuance, to the strength of the patient, the force of the disorder, and degree of deviation from an intermittent form. Venesection, once or more, in violent cases, or when there was any tendency to topical affection, was beneficial. I directed cooling, diluent, and acidulous drinks, until nature appeared to be making efforts to produce the sweating fit. When those efforts were apparent, the sweating was much accelerated, and the patient exceedingly relieved, by the use of small doses of an opiate, warm drinks, and warm applications to the feet. The symptoms of flatulency, which were at this time so painful and distressing, were by nothing so well removed, as opiates joined with some grateful aromatic oil, or distilled water.

When the intermission became distinct, and in urgent cases, where there was only a remission, I exhibited the bark, in all cases, unless opposed by the prejudices of my patients or their friends. I began the exhibition as soon as the sweating fit had subsided, and

continued it till the accession of the next paroxysm. From two drachms to one and one half ounce was taken in an intermission, according to circumstances. A suspension of the paroxysms never failed to be the consequence, the patient having no more than one, two, or at most three afterwards.

That the bark will suspend the paroxysms in this fever, is certain. And, that a continued use of it, or, what perhaps is better, an occasional use of it, together with a proper attention to diet, the state of the stomach and bowels, a due degree of exercise, and the regulation of the passions, for a sufficient length of time, will also perfectly obviate the tendency to relapse; is likewise a fact of which I have not the least doubt. But the cases are extremely rare in which we can obtain a proper attention to either of the above requisites to the desired effect. It must be acknowledged, that, in most cases, the bark did but suspend the paroxysms. It becomes a question, then, whether, in mild cases, it be best to cut the disorder short, or suffer it to run its course. I say in mild cases, for where there is the least suspicion of a doubtful event, the propriety of using the bark cannot be questioned. The tendency to relapse is equally great when the fever ceases spontaneously, as when cut short by the bark. The advantages obtained by the use of it are, that, increased debility is prevented, an exemption of a number of days from fever, with its concomitants, is obtained, and the danger of an accession of threatening symptoms is obviated. I know of no reasons of equal weight that can be opposed to its use, even in mild cases. But in cases of danger, a remedy which will, almost with certainty, avert a fatal event, ought not, on any consideration, to be dispensed with. In cases of pregnancy, a circumstance rendering the disorder a very dangerous one, I believe an early exhibition of bark affords the greatest chance of relief, even when no more than a remission can be obtained.

The DYSENTERY, especially in children, it must be confessed, seemed to defy the powers of medicine. I shall make some remarks on the remedies which I experienced the use of.

Purges; an assiduous use of them in the early stage of the disorder, was of the first importance. A judicious use of them in adults, would generally ensure a favourable event. Those most eligible, were Glaubers' salts, manna, castor oil, senna, and calomel. Each of these answered the purpose in some cases very well; but I found, after a thorough purging, at first with Glaubers' salts and manna, or the like, that repeated doses of castor oil gave greater relief than any other purgative. An abatement of the gripings, tenesmus, and frequency of stools, took place almost instantaneously after its operation, and to so great a degree, that many of my patients, after they had experienced the effects of this medicine, would, from their own feelings, determine when a repe-

tion was required, and request of me to administer it to them. Rheubarb I found to be a less eligible purgative in this disorder, than those which I have mentioned: I therefore soon quitted the use of it.

Emetics, in general, did not appear to be of any particular use.

Ipecacuanha, either as an emetic, purgative, or specific, did not appear to possess any particular salutary powers in this disorder.

Ripe fruits, the few that could be obtained, and drinks made from them, were certainly innocent, and probably of some utility.

Mucilaginous drinks, from gum arabic, marsh mallows, comfrey, the bark of what is called the slippery elm, and the like, I universally directed, and I believe with some good effect.

Antimonial, in combination with opium, after the use of purgatives, were certainly beneficial, particularly in the advanced stages of the disorder. I used emetic tartar, and antimonial wine, without being able to determine which of them deserved the preference. Either of these medicines, in conjunction with some of the preparations of opium, to which was sometimes added camphire, would procure an abatement of the frequency of the stools, a relief from pain and tenesmus, and some sleep. Opium alone produced no such effects, and was sometimes manifestly injurious: it increased the obstacle to the evacuation of the faeces, and of course the griping and tenesmus, and aggravated the symptoms of fever. I had no great experience of the effects of cerated glass of antimony; from what I had, and from the results of experiments made by other physicians, I am inclined to doubt its having any specific virtues in dysentery.

Clysters of mild mucilaginous substances, produced some relief from pain; with the addition of laudanum, they were, in the advanced stage of the disorder, a very valuable remedy.

Bark was not useful in simple dysentery, or even when complicated with fever, so long as the dysenteric symptoms predominated. In the convalescent stage, when the disorder assumed the form of diarrhoea, or when symptoms of the fever were present, it sometimes did good.

I attempted the sweating process, recommended by Dr. Mosely for the cure of dysentery. But when the sick are almost half of the time necessitated to be out of bed, and over the stool, it is impossible to procure and continue a salutary sweating. I soon gave it up as impracticable.

The celebrated nostrum of salt and vinegar was tried, and found, in mild cases, to have a salutary effect; but that it is not an infallible cure for dysentery, was incontestably proved by numerous experiments.

Astringents, of various kinds, were tried in all stages of the disorder, but, perhaps, in no instance with any good effect.

A vast multitude of nostrums, of various kinds, were recommended by quacks, and officious advisers, and frequently with

the most pernicious effects. No disorder, perhaps, affords more abundant scope for the exercise of quackery, than the dysentery. Generally fatal, beyond all other disorders, to children, who are the most obnoxious to its ravages, physicians are diffident in their expectations of success. The people naturally distrust those who distrust their own powers, and fly to the quack, who makes them positive assurances of relief. The perturbation of mind, on such occasions, is such, as to disqualify people from reasoning and judging. The scene passes rapidly off, and they are no wiser for their melancholy experience.

Since writing a part of the above, another season has past, which it may not be improper to give some account of.

About the time of the coming on of the sickness in 1796, and the other late sickly years, the bilious fever made its appearance again this year. It began in some families who had, subsequent to the sickness last year, removed to situations contiguous to the pond. It continued to attack people, within the boundaries which circumscribed the last year's sickness, until about the first of October. Those who had removed into the sickly territory since the last year's sickness, and persons who then escaped, were chiefly attacked. The former appeared to be particularly obnoxious to the disease. Between twenty and thirty persons, who had removed to situations contiguous to the pond, all sickened with bilious fever, without a single exception.

The circumstances of the season this year were not such as we should judge well adapted to produce, and give activity to marsh exhalations. The mill-pond, and other reservoirs of stagnant water, were, during the early part of summer, kept pretty uniformly full; and although the water in them, towards the latter part of summer, was lowered, it was at no time very low. There were, however, striking evidences of a vitiated atmosphere. The latter, I was informed, by persons who resided near the pond, was perceptible at a considerable distance; and from the universality of its effects, we must suppose the influence of the poisonous principle to have been, in no small degree, powerful.

There has been no instance of the fatality of the disorder in this town this year. The symptoms have been, in general, less violent than they were last year; in other respects, perfectly similar. The fever yielded with greater facility to the bark, than it has done in former years.

Dysentery has not appeared in any instance.

I have been informed of two persons, not belonging to this town, who, after residing a few days in the sickly territory, sickened with bilious fever, and died at their usual places of residence, which were where that fever has never been known.

Sheffield, Dec. 1, 1797.

ARTICLE III.

MEDICAL ESSAYS.—No. II.

INTRODUCTION.

AS a principal part of the evidence contained in the following Essay has been already published by Mr. Webster, in his Letters to Dr. Currie, it may be thought to require some apology from me for its introduction in this place. A simple statement of facts will, probably, be deemed sufficient.—The information from Mr. Paiba was communicated to Mr. Webster and me at the same time. The series of Letters in which Mr. Webster was then engaged, and the importance of the facts thus obtained, induced him to lay them immediately before the public. The probability of the existence of still further proof struck us both; and the inquiry after it, was relinquished to me. In executing this duty, (for such, under all the circumstances, I consider it,) I trust that some additional light will appear to have been thrown on this subject; while more leisure, and repeated conversations with Mr. Paiba, have enabled me to state every particular of his evidence more minutely and correctly, than could be done from a single conversation—in which it was impossible that some points of lesser consequence should not escape attention.

In respect to Dr. Chisholm, if it should be thought that I have spoken too freely of his conduct, I have only to reply, that the terms I have used are such as his conduct appears to justify; and such as he cannot complain of, if the facts be really as I at present believe them to be. But I have no quarrel with this gentleman, and I shall be as ready to withdraw those censures that I have passed upon him, should he convince me that they are unmerited, as I have been free to advance them; and in a manner equally public. An honest man can require nothing more: and this is a duty which every man owes to himself, as much as to the subject of his reprehension.

E. H. SMITH.

New-York, February, 1798.

On the ORIGIN of the PESTILENTIAL FEVER, which prevailed in the island of Grenada, in the years 1793 and 1794.

THE pestilential diseases which, within a few years, seem to have prevailed with uncommon mortality in many of the principal towns of the United States, have attracted universal atten-

tion, and conferred an unexpected, and perhaps undue importance on the question,—“Whether is the Yellow or Pestilential Fever, “as it has appeared in this country, since the year 1790, a disease “introduced or imported from abroad, or one generated among “ourselves by local causes?”

The advocates for the reality of importation, in every instance of the prevalence of the fever within the United States, maintain, that it is a disease of specific contagion, the product of other climates, and incapable of being generated in our own. In opposition to this sentiment, many publications have appeared, in Philadelphia, in New-York, and in various other places, till the public mind has become almost weary with the discussion. It is not intended to renew it at present. A single point, only, in the great subject, is meant to be elucidated. The present inquiry is limited to the examination of the evidence relative to a single instance of the prevalence of the Pestilential Fever.

The friends of the doctrine of foreign derivation, appear to have relied on no authority with more confidence than on that of Dr. Chisholm. It has become a sort of fashion among them to refer, with an air of triumph, to his testimony, as exhibited in his publication concerning a Pestilential Fever which prevailed in Grenada, in the years 1793 and 1794, especially in the former, and which he pretends to have been brought thither, in a ship called the *Hankey*, from the island of Bulama (or Boullam), on the coast of Africa. But although, were this writer's account of the origin of the disease to be admitted for truth, it would by no means decide the question as it relates to the United States, still, before such an admission is granted, it is obvious to inquire to what degree of credit it is entitled; whether Dr. Chisholm has carefully collected and displayed all the facts concerning the fever in question; and whether he may not, from inattention, or ignorance, or design, have materially mistated or withheld some important articles of information. If it can be proved, that his history of the Grenada fever is erroneous from either of these causes, the argument in favour of imported contagion will derive no assistance from what he has adduced. To shew that his account of the origin of that disease is erroneous and not to be depended on, is the design of this essay.

Towards the close of the year 1791, a number of gentlemen in England, associated for the purpose of forming a colonial establishment at Bulama.* The general objects of this colony were to

* Erroneously written *Boullam*, by Dr. Chisholm; who appears to have supposed, that Sierra Leone and Bulama were near each other; and the two settlements connected. Into this inaccuracy he was probably betrayed from the similarity between the names of Bulama and the shore opposite to Sierra Leone, which is called the *Boulom* or *Boulom* shore.

promote civilization and knowledge among the natives of Africa; to effect a substitution of the commerce in its commodities, for the disgraceful traffic in its inhabitants; and to dispose the people of that unhappy country to peace among themselves, and to husbandry and the useful arts. Its *particular* object was to raise cotton for the Manchester market. Some of the most respectable men in England engaged in this plan, either as simple purchasers, or proprietors and colonists; and after a general meeting of the subscribers had elected a governor and council, to manage the affairs of the colony, the governor and council appointed the following gentlemen as trustees for the association, to remain in England.

PAUL LE MESURIER, M. P. Lord Mayor in 1794.
Sir JOHN RIGGS MILLER, Bart.
Col. JAMES KIRKPATRICK.
DAVID SCOTT, Esq. M. P.
MOSES XIMENES, Esq.
GEORGE HARTWELL, Esq.

The names of these gentlemen are inserted here, as evidence of the respectability of the persons engaged in the enterprize.

The subscribers, or members of this association, raised about £9000. sterling, which was devoted to the purchase of conveniences for the colony; for obtaining the territory, meant to be occupied, of the natives; and for the purposes of trade among them. The intended colonists, to the number of 275, men, women, and children, exclusive of the crews of the ships, were to be transported in three vessels; two of them (the *Calypso* and the *Hankey*, of about 300 tons each) chartered for the purpose; the other a copper-bottomed sloop of 34 tons, belonging to the association.

The island of Bulama lies at the bottom of a deep bay, about fifty miles from the open sea, and opposite to where Rio Grande, a principal river of Africa, empties itself. The center of the island is computed to be in eleven degrees north latitude, and fifteen degrees west longitude from London: the place where the settlement was ultimately fixed, is about eleven and a half degrees north latitude. The circumference of Bulama is estimated at 120 miles, extending from east to west. The land rises gradually from the shore to the middle of the island, which is well-wooded, abundant in fine springs of water, full of game and wild animals of various kinds, and of a very fertile soil; being totally, as far as yet investigated, exempt from marshes and stagnant waters, and from stony ground. The tide is regular, and the spring-tides rise about 16 feet. The range of the thermometer, by daily observations, at noon, for ten months, is from 74° to 96° of Fahrenheit; and the medium heat 85°. The rains commence late in May, or early in June, and

continue till some time in October or November. On the whole, the situation is one of the most pleasant and healthy on the coast of Africa; though, like other tropical climates, not perfectly adapted to the constitution of natives of northern latitudes. Surrounding Bulama, but more particularly between it and the open sea, lie numerous islands of various magnitude, some larger, but the greater part not so large, forming an extensive Archipelago, to which the Bulama colonists gave the fanciful name of the *Hesperides*. At this time Bulama was uninhabited, but was the property of the Canabacs, a powerful, warlike and ferocious nation, residing on a neighbouring island.

Soon after the vessels put to sea, they were separated by a storm; and the Calypso, after touching at Teneriffe and Goree, arrived at Bulama, about the end of May, 1792, and near a fortnight before the Hankey. The whole of the people were in health when they sailed. The Calypso lost a man and two children on the voyage.

On the arrival of the Calypso, the governor landed some of the people; and while he was absent, with a small party, to explore the island, the Canabacs, mistaking the design of the colonists, surprized those who were left behind; killed and wounded a number; and carried off others as prisoners. This unfortunate occurrence alarmed the others so much, that the governor deemed it prudent to sail back to the island of Bissao (one of the Archipelago), where there is a Portuguese settlement. Here he found the Hankey and the sloop, who had arrived after touching at Teneriffe and St. Jago. While at Bissao, they conciliated the Canabacs; recovered those who had been made prisoners; and from thence returned in company to Bulama, which they soon after purchased, together with the adjacent island of Arcas.

The hurry with which the colonists had quitted England, some delays created by the government at home, in their departure, and their number, which exceeded what was at first expected, prevented their carrying out with them many conveniencies essential to the commencement of an establishment like the one proposed, and retarded their arrival till near the beginning of the rainy season. Some of the Calypso's people had already fallen sick when the Hankey arrived; and as there were no accommodations provided on shore, both sick and well were confined to the ships. The rains now setting in, obliged them to frame a sort of covering to protect them from the weather. In this confined situation, the heat and moisture were very uncomfortable; a due degree of cleanliness could not be preserved; and the disappointment and chagrin of most, with unusual labour, soon introduced sickness. In the Calypso, where the people were most crowded, it was now most prevalent; it soon affected the colonists on board the Hankey; but

it was not till a long time after, that any person sickened that belonged to the sloop, which was actively employed, and contained none of the intended colonists, but its own crew, or such as were occasionally on board to perform some service for the association. At length, the discontents of the colonists rose to such a height, that the greater half of them resolved to relinquish their design and return to England. Some few of them remained for a time at Bissao, but the principal part set sail in the Calypso.

The Calypso left Bulama about the 23d of July, for Sierra Leone; where the people were in hopes of finding accommodations till the cessation of the rains. Their passage was uncommonly tedious and tempestuous; many sickened on the way; and two or three died. When they arrived at Sierra Leone, they were disappointed of the expected accommodations on shore; and being more crowded now than at Bulama, in a warmer latitude, the season further advanced, with additional disappointments, and wholly destitute of occupation to divert their thoughts from their pressing difficulties, it is not surprising that the sickness on board the Calypso increased, and that a greater number perished here than had died out of both ships, previous to her departure from Bulama.

Concerning the sickness which carried off the colonists, both at Bulama, at Sierra Leone, and on the home passage of the Calypso, it may be remarked once for all, that it was by no means of one kind, as the readers of Dr. Chisholm would be led to suppose. Few, if any, escaped altogether. Some had regular intermittent fever, (which is the fever of the coast,) of various continuance, from a few weeks to several months: others had a violent fever, which terminated favourably or fatally in one, two, three, four, five, or six days; or which lingered out, after its first violence, as many weeks: some had diarrhoea and dysentery; and others fell martyrs to the indiscreet use of spirits and opium, as preventatives: and all these varieties happened on board the same ships, without any obvious difference in the exposure of the individuals to the causes of disease. Yellowness of the skin was a common appearance in the sick; and some of those who died at Sierra Leone had black-vomit; and this symptom also appeared at Bulama in the latter part of the season.*

The Calypso left behind, at Bulama, with the Hankey and the sloop, eighty-seven persons, viz. forty-nine men, thirteen women, and twenty-five children, well supplied with provisions. But be-

* Mr. Paiba informs me, that a girl recovered who had been very ill on their passage to Sierra Leone. She had been speechless several days, and for three or four days apparently dead, after having had the black-vomit.— This I believe was the first time that this symptom occurred among the Bulama adventurers.

fore any further account is given of the fate of this remnant of the original colony, it is to be observed, that the *Calypso*, the ship on board of which the sickness commenced and was most severe, both at Bulama and at Sierra Leone, arrived at London on the 14th of November, 1792, with between eighty and ninety colonists; that she lost thirteen people between Sierra Leone and London; that five others died out of her after she arrived at her moorings in the Thames; and yet, that no disease was ever propagated, or reported to be propagated by her, in London; nor do there appear to have been any obstacles opposed to her entrance, or to the landing of the people that came in her, or of the quantities of goods that they brought back with them—though this ship had only been subjected to a partial, and by no means to a thorough, cleansing, previous to her leaving Sierra Leone.

After the departure of the *Calypso*, the remaining colonists applied themselves with great assiduity, together with such of the natives as they could hire, to the preparation of accommodations for themselves on shore. The difficulties they had to encounter were numerous, considerable, and discouraging. Many of them were unaccustomed to labour; the sickness still continued; they were obliged to keep up a constant watch, for fear of their ferocious neighbours, the Canabacs; and they worked in the rains and heat. During the whole of the rains they had no breezes; so that the weather was close, sultry, and very oppressive. They succeeded, however, in constructing a considerable block-house, within which lodgings were prepared for such of the people as resolved to continue permanently at Bulama. But no particular difference was observable between the condition of those who lived on shore, and those who remained in the *Hankey*. Some of the people in both situations were sick; with no variations in the nature of their complaints, ascribable to the circumstance of being out of, or in, the vessel.

The official return of the acting governor of the colony, from the sailing of the *Calypso* till the 22d of November, when the *Hankey* left Bulama, is as follows:

	<i>Men.</i>	<i>Women.</i>	<i>Child.</i>
Left behind when the <i>Calypso</i> sailed	87	49	13
Returned from Sierra Leone	6	2	3
	—	—	—

93

* In this account of those who returned from Sierra Leone, Mr. Paiba informs me that there is a small mistake. He makes the number to have been eight, viz. five men, one woman, one infant, one servant boy. The difference is not material, as the rest of the return is accurate.

MEDICAL REPOSITORY

477

Of these there died 26 of fever,	12	5	9
1 of fever and flux,	1		
2 of consumption,	1	1	
1 of lunacy,*	1		
1 mortified hand,	1		
1 drowned,	1		
1 worms,	1		
2 complication of disord.	2		
1 hooping-cough,			1
4 disease not specified,	3		1
<hr/>	<hr/>	<hr/>	<hr/>
40	23	6	11

From the preceding list of deaths and diseases it is apparent, that though the mortality was very great, it was not wholly occasioned by fever. It may be added, that under the title *fever* are included those who died of it under *every* form; and whether they were hurried into their graves after a sickness of two or three days, or gradually sunk into them from the oppressive debility induced by an intermittent of several months continuance.

The time for which the Hankey had been chartered having expired, Captain Coxe prepared to sail for England; and with him several of the colonists—among others Mr. Paiba (one of the council) and his lady; making, in the whole, nineteen or twenty persons; all of whom, excepting Mr. and Mrs. Paiba, and a woman, were unwell. But before the Hankey put to sea, all the bedding of the sick was thrown overboard or destroyed; the ship was washed from stem to stern, both above and below, with salt-water, and then with vinegar and water; and the purification was completed by thoroughly fumigating her with tar, pitch, and gunpowder. In this clean condition they bade farewell to Bulama on the 22d of November, 1792; but in attempting to pass through the channel near to the entrance into the open sea, in a dark and foggy night, they got aground on a sand-bank, upon the north side of the island of Formosa, or Warang, belonging to the Bijugas, who are represented as Cannibals. The extreme terror excited by this accident, was not calculated to improve the health of the people on board the Hankey; so that, when it became necessary to take measures for their security and deliverance, only four men were found in a condition to do duty, and all of these had intermittents.

* By *lunacy* here is not to be understood *mania*. It several times happened that persons who had been violently attacked with fever, were left by it in a state of derangement more like *idiocy* than madness. In this wretched condition they frequently lingered some months, and then died. This the governor has denominated *lunacy*. Mr. Paiba believes that the four persons whose disease is not specified, must have perished in this miserable condition.

Vol. I. No. 4.

F

With them, however, and his lady, Mr. Paiba sat off in an open boat, for Bissao, to obtain assistance from the Portuguese settlement. Thither he arrived, rowing through rains and fogs, in a leaky boat, after being out two nights and a day; and having attained such help as he could, returned to the Hankey, got her off, and carried her back to Bissao. On the passage there, eight persons died of those that belonged to this ship. At Bissao they refitted; and the Hankey *was a second time purified as completely as she had been before leaving Bulama.*

At length, all things being ready, the Hankey left Bissao, and by fortunate pilotage passed through the channel without accident, cleared Cape Roxo, and stood off to sea. There were then on board, Capt. Cox, his two mates, boatswain, two seamen, and the cabin-boy, *crew*; and Mr. and Mrs. Paiba, a servant boy of theirs, a woman, and a little girl, *passengers*; in all twelve persons. The little girl died when they were at St. Jago; but, at this time, none of them were sick, but with debility and slight intermittents. A circumstance which marks the time of their sailing is, that they observed the festival of Christmas on this voyage, which was only of five days.

The Hankey arrived at St. Jago on the 30th or 31st of December; but, in attempting to enter, unfortunately got among the rocks in the Bay of St. Francis; from which she was cleared, after some days, by the assistance of two boat-loads of Americans, and thirty or forty of the natives, and brought into Port Praya. At this place the Hankey continued about two months. During this stay, all the people recovered their health, except the child who died, and Capt. Cox, whose fever being irregular, and irregularly treated, he was sometimes well for several days together, and then had a relapse of about the same duration. Bark and the usual auxiliaries never failed to arrest the progress of his fever and ague; but, as is commonly observed, indulgences in free living, or in exertions of business or pleasure, caused it to recur.

From the moment of the arrival of the Hankey, till her departure, no other person that belonged to her was unwell with any ailment of consequence; her crew and passengers mixed without suspicion, and with perfect freedom, with the inhabitants of Port Praya; and received them on board, where they had a number of entertainments, of which the governor of the island and several of the principal people partook: nor was there ever any sickness occasioned by all this long intercourse, or suspected of being excited by it. Indeed, no sickness prevailed at St. Jago, during the Hankey's stay, except the common ague and fever of the place. And, certainly, it must be thought very extraordinary, that a ship which retained such deadly infection as to poison the whole town of St. George in Grenada, several months after leaving Bulama,

should not only not have affected a single person at St. Jago, during two months continuance there, and when a daily intercourse was kept up between her and the town of Port Praya, but should have been equally harmless to two boat-loads of Americans, fresh from a northern climate—especially as, were Dr. Chisholm's account to be received as truth, the contagion must be believed so active as to have destroyed the first person who came on board after her arrival at Grenada, and so diffusive as to extend, from vessel to vessel, through the whole harbour.

When the Hankey had been about a month at St. Jago, Commodore Dod arrived there in the Charon, a 44 gun ship. He had been directed by his government to afford such assistance to the infant colony at Bulama, as was consistent with the national service. In consequence, he had previously dispatched the Scorpion sloop of war, which was under his command, to visit Bulama, and return to him at St. Jago, by a particular time. The Scorpion proceeded accordingly, and the colony was mustered by Capt. Ferris, the commander, on the 11th of January, 1793.

Mr. Paiba was not at Port Praya, when the Charon arrived, but in the country. But he immediately repaired to town, and paid his respects to the Commodore, to whom he unfolded his situation, and that of the people who were with him, and requested such assistance as the Commodore could afford them. It was soon discovered that the Hankey would not be able to obtain such an addition to her crew, as would render it prudent to attempt to navigate her back to England; and notwithstanding Capt. Coxe's desire to undertake the voyage, (as his people were all well when the Charon left St. Jago,) the Commodore expressly directed him to make the best of his way to the West-Indies. The Scorpion not arriving at St. Jago by the time expected, the Charon, after a stay there of a fortnight or three weeks, and after sparing two seamen to the Hankey, departed for the coast of Africa; leaving an order for the Captain of the Scorpion to furnish her with two additional sailors. A few days after, the Scorpion came to Port Praya; and having reluctantly complied with the order, sailed again, soon joined the Charon, and they visited the several settlements on the coast in company.

As much stress is laid by Dr. Chisholm, on the intercourse between the Charon and the Hankey, it is proper to state particularly, what it amounted to.

As the people of the Hankey, and particularly Capt. Coxe, had not perfectly recovered their health when the Charon arrived, the surgeons of that ship were allowed to visit the Hankey, and to direct what was proper for them. But neither of these gentlemen were unwell in consequence. Commodore Dod being a strict disciplinarian, and beside exceedingly fearful of contagion,

never permitted his people to go on board the Hankey, except that in one instance, he suffered two of his men to assist in some repairs on her rigging. But before this, and indeed previous to any communication with that vessel, he had sent on board of her a quantity of powder; and the *purifications of Bulama and Bissao* had been repeated.

The barge of the Charon, by the Commodore's strict orders, had no other connection with the Hankey than simply this:—It was his custom to send it every morning to the Hankey, for Mr. and Mrs. Paiba, who usually spent the day with him, and returned at evening. When the barge came along side of the Hankey, it was kept waiting till Mr. and Mrs. Paiba were ready; when they entered it, and were immediately carried on board the Charon. At evening, they returned to the Hankey; and as soon as they left the barge, the people went back as before. So that the barge-men never were in the Hankey; and excepting the surgeons, the two mariners who were a short time (part of one day) employed about the rigging, Mr. and Mrs. Paiba, and Captain Coxe, who was three or four times in the Charon; no other people, belonging to either ship, had intercourse or communication with the other; and the connection with the Scorpion was still less, and of less duration. Nor was there ever any sickness on board either the Charon or the Scorpion, during their stay at St. Jago; and if any broke out afterwards, it was in all probability generated in those vessels, during their continuance on the African coast, by the same causes which occasioned it in the Calypso and the Hankey, and which so generally give birth to it in ships which leave cold for hot climates: for it is incredible, if any disease were communicable by the Hankey, that she should not rather communicate it at first, than after her long stay, and to the Charon before her purification, than just as the latter was ready to sail, so that it did not appear till after she had reached the coast of Africa. All these circumstances, therefore, being viewed in connection, there is every reason to believe that Commodore Dod was mistaken, in attributing the disease of which his men died, to contagion received from the Hankey; and that he consulted, in this instance, rather his fears and his prejudices, than his reason and the facts before him.

The latter end of February, or beginning of March, the Hankey left St. Jago, and proceeded for the West-Indies. Her crew and passengers were the same as before, with the addition of two mariners from the Charon and two from the Scorpion—one of which last was unwell when he came on board the Hankey, but able, at that time, to do duty. He grew more and more unwell as they proceeded, and actually died when they had been ten or twelve days at sea. Capt. Coxe, who was still unwell when the

Hankey left St. Jago, recovered his health before they reached the West-Indies, (though he afterwards had a return of his disorder,) and all the others were perfectly well, (notwithstanding the hard duty they had to perform,) and continued so.

After a passage of nineteen days, the Hankey arrived at Barbadoes, where she remained three or four days. While there, the people of the place came freely on board, and those of the ship all went on shore, and mingled as usual, with the inhabitants; but no disease was communicated, nor did any prevail at Barbadoes that year, like that which appeared at Grenada.*

From Barbadoes, the Hankey proceeded to St. Vincent's, where she made a stay of a day and a half, and where the same free intercourse took place between the people of the ship and of the island, that has been noticed in respect to the other. But no pretence ever was set up, that a contagious disease, or any disease, was communicated to the inhabitants in consequence; nor did the sickness which appeared at St. Vincent's the same year, commence till some time after that of Grenada had become general. It was then conjectured, that the Grenada fever had been imported into St. Vincent's; though, more probably, both arose from local causes, which might be more active at one place than at the other.

From St. Vincent's, the Hankey sailed to Grenada; where she arrived late in the month, or not till after the 19th of March: a month after the time fixed on by Dr. Chisholm. She entered, as he says, into the Bay of St. George's; but was soon after carried round into the Careenage. The distressed condition of which Dr. Chisholm speaks in his account, arose solely from the want of sufficient hands to navigate the ship, and the consequent fatigue of those on board; for, at this time, all, even Capt. Coxé himself, were able to do duty, and were in health.

Immediately on his arrival (in consequence of a recommendation letter from Commodore Dod), Mr. and Mrs. Paiba were invited about 14 miles into the country, to the plantation of an English gentleman, at whose house they resided during their continuance in the island of Grenada. A circumstance which may be considered as sufficient evidence, that Commodore Dod had no apprehensions of the liability of any person to contract disease from the people of the Hankey, at the time of his departure from St. Jago. As a confirmation of the truth of this supposed opinion of the Commodore, it may be observed, that Mr. Paiba went several times on board of the Hankey, mixed unrestrainedly with the

* One man belonging to the Hankey even remained behind, and resided at Barbadoes for some time—without occasioning any pestilence. His name is Curwood; I am informed he now lives at Albany, and will corroborate all the material parts of Mr. Paiba's statement.

towns-people, was at Dr. Chisholm's house, at the houses of most of the reputable merchants in St. George's, and yet, neither himself, his family, the friends with whom he resided, nor the families with whom he principally associated, ever had the disease; a fact very extraordinary, were Dr. Chisholm's notions of the virulence and extreme activity of the contagion supposed to be introduced by the Hankey, well-founded.

Still further to remove all suspicion respecting this ship, it is proper to remark, that she had no cargo on board, nor any thing but her few stores, and some goods belonging to Mr. Paiba. These goods were landed at St. George's, and stored at the house of a gentleman of that place, a friend of Mr. Paiba's, (a Mr. Napier*), where they continued unmolested; and no member of Mr. Napier's family suffered from the fever. The Hankey, in fact, was in ballast; which consisted of *green wood*, put on board at Bulama or Bissao; and which, being *dry* when she arrived at Grenada, and of course no longer useful for the original purpose, was thrown out there, and sold for firing. From such a lading as this, the most inveterate disciple of Dr. Chisholm will hardly expect to extract "contagion, pestilence, and death."

From a comparison of the preceding narration, with the history delivered by Dr. Chisholm concerning the Hankey, the reader will probably find ample room for doubt, as to the remaining statements of that much-quoted author. The facts and reasonings which are to follow, will perhaps succeed in removing any uncertainty which may yet remain. To this end, the reader is supposed to bear in mind, the several parts of Dr. Chisholm's publication, as he proceeds with the writer in the present investigation.—It is necessary, in the first place, to exhibit a correct statement of facts relative to Capt. Remington.

The Hankey had been at Grenada at least a month, and the sickness was universally known to be in the town of St. George, when Capt. Remington visited that ship. Luckily, Mr. Paiba was then on board, and remembers all the circumstances. These are his words.

"He (Capt. Remington) had been all day and night coming from Grenville Bay, and had been wet through. He slept on board in his clothes; and went in an open boat, *the next day*, back to his ship: enough to kill any one in that climate."†

* Mr. Napier's house is on the Bay-side of the town of St. George.

† Manuscript communicated to me by Mr. Paiba, and now in my possession. Mr. Paiba being the principal part of the time on shore, is not able positively to contradict the assertions of Dr. Chisholm, concerning the crew of the *Defiance*—but from the foregoing narration; from what is proved respecting the case of Capt. Remington; and from what will be mentioned hereafter of Dr. Chisholm's conversation with Mr. Paiba; I have no hesitation to disbelieve the whole.

A mistatement of this fact, so surprizingly gross and enormous as that of Dr. Chisholm, unavoidably inspires something more than doubt concerning his whole narration. In this situation, it is proper, as far as may be possible at this distance of time and place, to institute an inquiry into all the circumstances of the memorable disease that prevailed in Grenada in the year 1793.

1. The town of St. George, in the island of Grenada, where the pestilential disease raged in 1793, is situated on a narrow strip of land, bounded on one side by the Bay of the same name, and on the other by the Careenage.

The Careenage is a long inlet or arm of the sea, running up on the south and east side of the town of St. George. It is low; little moved by winds; and nearly stagnant—as the tide never rises or falls there more than four, five, or six inches.

The shore of the Careenage, on the town side, is remarkably low, narrow, filthy, and crowded by numerous little buildings, chiefly of wood, which are separated only by lanes, situated on or near the wharves, and inhabited by the lowest of the people, ill-accommodated, and devoted to intoxication and every species of debauchery.

At the head or east end of the Careenage, is a large *marsh*; which, at all times, but especially at low water, sends forth the most noisome and offensive smells.

Into this inlet, vessels are generally brought to refit and careen, and indeed for protection, as they are perfectly safe from the winds. At the time of the Hankey's arrival, it appears to have been unusually crowded with shipping; and that ship was brought round from the Bay, into the Careenage, very soon after she reached Grenada.

2. This being the place where the sickness first shewed itself, and whence it extended to the neighbouring parts of the town, it may be proper, in the next place, to examine whether there was any thing remarkable in the seasons of the year, which should render the Careenage more than commonly liable to disease. The reasons which the preceding part of this essay afford for not acquiescing in the narrative of Dr. Chisholm, make it equally improper to receive his opinions on this subject without further inquiry: especially as the result of such inquiry seems to confirm a contrary position, and authorize a conclusion, that the weather of 1793 was favourable to the generation and spread of pestilential diseases in the vicinity of the Careenage.

The month of February 1793, was generally rainy; which, Dr. Chisholm remarks, "is an uncommon circumstance." March and April were mild, and more than half of May, "dry and dusty." During all this time, the wind was easterly and southerly, i. e. was from the very points to blow over the *marsh*, at the

head of the Carenage, into that part of the town which lies directly upon that inlet, and on to the shipping there at anchor. June, again, was very rainy: scarcely a day passed without rain: wind still from the south-east. The first part of July was *dry*; the latter part *wet*; and the atmosphere foggy and loaded with vapour. August was *dry*—"the atmosphere generally excessively close, sultry, and loaded with vapour. The winds were very variable; but for the most part a calm." The four following months remarkably rainy and tempestuous.

Whoever will take the pains to compare the weather of 1793, with that of three other years, as reported by Dr. Chisholm, cannot but be struck with their dissimilarity with that year, in this particular. It is a fact universally admitted, that constant *wet*, or constant *dry* weather, are both unfavourable to the generation and spread of pestilential fevers. It matters very little, in respect of the health of those who live in its vicinity, whether a *marsh* be completely drained, or constantly overflowed. It is from its humid surface, exposed to the action of heat and air, that pestilence arises. Now it will be observed, in Dr. Chisholm's Diary, that, in the year 1784, February and March were *dry*; and April, May, June, July, August, September, and October, were very rainy. In 1785, February, part of March, and all April, were *dry*; May, June, and July, rainy. August was chiefly *dry*; but September was wonderfully rainy and tempestuous—so that it probably destroyed any beginning epidemic, as the weather continued rainy all the fall. In 1786, February, March, April, May, and part of June, were mostly *dry* and pleasant. The remainder of June, July, and August, were *rainy*; September, and the first week in October, remarkably *dry*; but the rest of the season as remarkably *wet* and tempestuous.

From this comparative view of the weather, at Grenada, in different years, it appears, that whereas the seasons in general are singularly marked by long continuance of very wet, or very dry weather, in the year 1793, after the preparation of a dry spring, they were distinguished by alternate rains and dry weather—a succession the most favourable to prepare and bring into action the morbid miasmata of the neighbourhood of St. George: and to this, in conjunction with other causes, enumerated in the course of this Essay, may be ascribed the peculiar severity of the Yellow or Pestilential Fever of 1793. Even Dr. Chisholm himself acknowledges (p. 103) that "it had been urged by some, that the disease arose from the state of the atmosphere; and that human contagion could not give rise to it, as it was so prevalent in distant and distinct places at the same time."—It belongs to another part of this paper to assign the probable motives of Dr. Chisholm for maintaining that the fever was imported into Grenada: certain it

is that he avowed a different opinion to Mr. Paiba, to whom he freely declared that he could by no means trace the disease to the *Hankey*; and that he believed it to be of local origin,—owing to the unhealthy condition of the Carenage, and to the particular prevailing winds: and, to confirm this notion, he informed Mr. Paiba that a similar disease, from the same cause, though in a less degree, had existed in St. George's, some years before:

3. If the situation of the Carenage, and the weather of the year 1793, appear to countenance a different opinion than that of Dr. Chisholm, as to the origin of the Pestilential Fever of Grenada, a no less formidable argument may be deduced from his own representations concerning the time of its first appearance in that year. For in p. 91. the Dr. remarks, that “in the short space of time from the beginning of March to the end of May, 200 of about 500 sailors, who manned the ships in the regular trade, died of this fever.” By this it appears that the Fever in question broke out as early as the beginning of March. The disin-
genuousness of this author is particularly evident from this quotation, if the period of the commencement of the disease be correctly assigned. And that it is so, is probable from the difficulty of concealing the fact; as there must have been thousands of witnesses to the progress of the Fever. When, therefore, it was thought proper to fix the odium of introducing the disease upon the *Hankey* (a project of which Dr. Chisholm seems originally to have had no idea), it became necessary for him to assign an earlier date to her arrival. Now, that the *Hankey* did actually not arrive till towards the latter end of March, is verified by the concurring testimony of Mr. and Mrs. Paiba, and of Mr. Bell of this city, who happened to be in Grenada about that time, and was personally acquainted with Mr. and Mrs. Paiba in that island. So that, if the disease commenced as early as the first of March, Dr. Chisholm has inadvertently disproved his whole account of its origin; and it is clear that the *Hankey* (which did not arrive till after the 19th of March, instead of February) could not have introduced it.

Another inadvertence of Dr. Chisholm, relative to this point, is, that though he speaks of Captain Remington as having been the first of all who were seized with the fever, which was next communicated to the crew of the *Defiance*, &c. yet he afterwards (p. 223.) details a case, which he declares “is inserted chiefly from the circumstance of its being the *first* which occurred.” But this is neither the case of Captain Remington, nor of one of the crew of the *Defiance*; and if, by the word *first*, it be supposed that Dr. Chisholm means the first that occurred in his practice, the evidence may be demanded of the truth of his assertions respecting Captain Remington and the *Defiance*, with the circum-

stances of which, it seems, he was not personally acquainted.—It will be remarked, also, that the date of this case is omitted—perhaps inadvertently.*

4. What is naturally inferable from the state of the Careenage, the peculiar weather of 1793, and from the contradictory assertions of Dr. Chisholm himself relative to the commencement of the Grenada Fever, is powerfully corroborated by his own account of the progress of the disease, and of the order in which it successively appeared among different descriptions of persons. Like our own Fevers, it was mild or severe, general or partial, in exact proportion to the accumulation or absence of the local and individual circumstances which favoured its attack and extension.

It commenced among *sailors* lately from northern climates, and who were constantly employed in the midst of this horrible Careenage, in this unfriendly season. A moment's attention to Dr. Chisholm's enumeration of predisposing causes will remove any doubt why the disease should first shew itself in this place, if any doubt be entertained. Numbers of young men, from our own country, are every year carried off by the operation of much less active causes. "It must not remain unconsidered, (p. 92.) that "the predisposition of the class of men among whom it happened, was very great. The sailors were from the age of fifteen "to fifty; and the circumstances which appeared to predispose "them more strongly than other men to the action of the contagion, were violent exercise in the sun; the immoderate use "of undiluted new rum; bathing in a state of intoxication, "and often when violently heated; sleeping on deck during the "night. Other circumstances, which did not depend so much "on their own prudence, no doubt, contributed very much to "give the disease so very fatal a tendency: the damp heat between decks; the excessive filth of most of the ships; and "the uncleanly state of the persons and clothes of the men themselves." Now what madness or malignity must possess a man to seek elsewhere for causes sufficient, in connection with climate, season, and the local situation of the people, to breed the most terrible of plagues? In this paragraph, are enumerated all, and more than all, the causes which operated at Bulama, in a situation more healthy; and in a climate and season no more unfavourable to health. Nor need there be any surprize excited by the superior virulence of the Grenada fever, to that which prevailed among the colonists at Bulama.

From the sailors and people of the shipping lying in the Careenage, the fever spread itself among the residents on the immediate verge of this inlet: among human beings enveloped in impure

* The dates of all the other cases are carefully noted.

air, buried in filth, and devoted to prostitution and drunkenness: amid wretched habitations, huddled together, exposed to the noxious exhalations and noisome effluvia of the Careenage, directed more particularly upon them by easterly winds; and nearly sheltered from the purer breezes of the open sea, by a hill whose steep ascent conducts into the principal and best built part of the town.

"It will not appear extraordinary," says Dr. Chisholm, p. 94. "that the lower classes of men, and those more especially of loose and debauched manners, should be the most subject to this disease; their greater exposition to the influence of infection, which their business as tradesmen rendered necessary, contributed not a little. But the description of men by far the most obnoxious to this contagion, and who suffered most from it, were those lately arrived from Europe; and of them, those who had never before been in a hot climate."*

The same circumstances occur in the United States: emigrants, especially those who are poor, ill-lodged, ill-fed, and intemperate, suffer first; and next, those of our own people who most resemble them in situation and habits.

After the appearance and considerable prevalence of the fever on shore, the garrison became affected. Dr. Chisholm lays much stress on its vicinity to the Hankey, and a consequent supposed communication between the soldiers and that vessel. But, for the general reasons that exist for suspecting the testimony of this writer relative to the origin of the disease, this opinion does not seem deserving of any particular credit; especially when it is recollected that the garrison was also near the Careenage, and that the disease "proved fatal only to recruits who had lately joined." Nor will it deserve to be considered as "a circumstance rather extraordinary," that the fever should break out among "the detachment of the Royal artillery," though "that corps were quartered in a situation far removed from the focus of infection," "when the reader is informed that the station of this detachment was directly upon or by the side of the offensive *marsh* at the head of the Careenage; that "the predisposition of the men to receive infection, as far as that could be induced by excesses in drinking and other irregularities, was by no means less than that of the sailors, &c." and that, though of those who had been long enough in the country to become seasoned to it few died; yet, of twenty-six new-comers, twenty-one fell victims to the fever.

The table which Dr. Chisholm has inserted (p. 100.) of the order in which the Pestilential Fever seized on the different classes of persons, or of the degree of their exposedness to the disease and to a fatal termination of it, perfectly corresponds with what has

* See likewise page 202, & sequent.

uniformly been observed in the United States, and is in all respects such as might rationally be expected, from the operation of those local causes which will, sooner or later, be generally admitted as the originators of these formidable disorders.

Nor is the progress of the disease, in regard to place, less evincive of its local origin. It commenced at the Carenage; extended over the adjacent wharves; proceeded to the lanes and narrow alleys and streets in their vicinity; and thence into the nearest parts of the town. But it never obtained any general prevalence in the neighbouring plantations, nor even in the higher, better built, wealthier, more airy, and cleaner parts of the town.

The situation of the buildings on the Carenage, in respect to the rest of St. George, resembles that of Water-street and Front-street in Philadelphia, the ascent from one to the other, in both cases, being about equally steep; and the Bay-side of St. George may be compared to the North-River-side of New-York, which has hitherto been exempt from pestilential diseases: an exemption of which it may perhaps be deprived, at some future period, by increase of business, and the erection of a few more streets on made-ground. The width of the two cities (New-York and St. George) is also about the same. But, as in New-York; so it will be remarked, that in St. George's, those whose business led them to spend most of their time in the neighbourhood of the Carenage, were by no means safe from the fever, though their houses were in healthy situations. Still, as they sickened in more pleasant places, and were in every respect well-accommodated, the disease in them was, as Dr. Chisholm observes (p. 93.) "infinitely milder," and could scarcely be called the same. Yes, this terrible pestilence was reduced to an ordinary fever, or a disease of common malignancy, when it happened among the wealthy inhabitants of St. George, who resided in high, clean, and well-ventilated parts of the town. The same has been the case in New-York; and has, in not a few instances, led practitioners to maintain the existence of a variety of fevers, and to suppose that the gentle means which succeeded perfectly in these mild cases, were equal to a combat with the most malignant forms of pestilence. But passing this error without further notice at present, it is evident, from several parts of Dr. Chisholm's publication, that considerable variety was observable in the symptoms of the fever he describes; and that in some cases, it bore a sufficient resemblance to the Yellow Fever of other seasons, to incline many persons to believe it not altogether distinct. "When this disease first appeared here, (p. 146.) and for some time after, the prevalent opinion was, that it was the Yellow Fever of the West-Indies, engrafted on the European Jail-Fever." "In some

"protracted cases on shore, and in some among the sailors, which "might have been a combination of the Pestilential and Yellow "Fevers, this symptom (*a yellow skin*) appeared about the fifth, "seventh, or ninth day." With the reader who is apprised of Dr. Chisholm's motives for wishing to establish a distinction between the ordinary Yellow Fever of the West-Indies, and the Grenada Fever of 1793, these concessions, notwithstanding all the author's endeavours to destroy their force, will have their due weight. The philosophical inquirer will find little difficulty in conceiving that a disease, which, in ordinary seasons, he is told, is endemic and sporadic, in Grenada; (Chisholm, p. 147.) and "is evidently "caused by marsh effluvia, heat, violent exercise in that heat, "thick, hot, moist atmosphere, night air and dews, and the abuse "of spirituous liquors"—may be exalted to a higher degree of virulence and activity by a more than usual combination of these causes, in a season favourable to pestilential disorders. He will even derive new reason for his conviction on this point, from the assertion (in the same page) that the Yellow Fever is *never known to be contagious*, when he reflects how intimately the immediate sources of fevers are connected with those complaints, and how remarkably the progressive extension of pestilence compares with the gradual impurification of the ambient atmosphere.

5. A still further confirmation of the opinion meant to be advanced in this essay, in opposition to the assertions of Dr. Chisholm, concerning the origin of the Grenada fever of 1793, may be discovered in the effect which was produced in the sick, by their removal from the ships and low places where they first became sick, and were still exposed to the operation of the original causes of the disease. While they remained in these situations, the pestilence assumed its most threatening aspects, and all the skill of the physician was frequently unable to rescue them from its fury. If subdued to a certain degree, it yielded but in the most gradual manner, and the convalescence was tedious and protracted, from the extreme difficulty of generating new habits, while the system was continually operated on by the causes most powerful to counteract them. But, when removed to a high and airy situation, the sick found an almost immediate mitigation of their worst symptoms, and the return of health and strength was nearly consentaneous with the disappearance of disease. Were this not the fact, it would well deserve our astonishment. But in what respect does this differ from what is ordinarily observed in the commonest and slightest forms of fever? Who is ignorant that the attacks of an intermittent are more readily obviated, when the patient leaves the neighbourhood of a marsh, and fixes himself on a dry and elevated station?

6. Another circumstance deserving of attention, is the obvious

difference between the Grenada fever, and diseases of specific contagion.

It is true, that very great dissimilarities are observable in the degree of violence with which the small-pox and measles affect different individuals; and that these diseases are modified by situation, diet, constitution, cleanliness, &c. But still they are contagious. It is not found that, notwithstanding these circumstances modify, they extirpate or destroy their contagious property. Who is able to produce an instance of the total disappearance, within a few weeks, of an epidemic small-pox or measles, where they have not been driven away by new epidemics, or where the subjects of their attacks were not exhausted, or had not been removed from their reach by social regulations?

If the Grenada fever was of the same nature, why did it not extend to every part of the town of St. George, and over the whole island? What was there, in so warm and equable a climate to arrest so suddenly and totally a disease of specific contagion? Why was its course always impeded or stopped by a pure air and personal cleanliness? Why did it respect natives more than foreigners? of the first, those whose situations were, on other accounts, most eligible? of the last, those who were most careful and temperate? Does any one suppose that either of the diseases above-mentioned—that the small-pox or the measles—would have been so complaisant?

The true causes of these varieties are sufficiently evident. Men, in general, can never materially change their climate and situation, without exposing themselves to a correspondent alteration in their health. Those who remove from northern to southern climates, must expect to experience some morbid variation of it. Its degree and danger will depend on the extent and suddenness of the change of climate, and on the peculiar circumstances of place and person, food and season. The disease to which men, who go from the higher latitudes of Europe and America to the West-Indies, are commonly exposed, is that form of pestilence which is called Yellow Fever. This disease is much more mortal in some years than in others; being, like other general diseases, modified by season, &c. To so high a grade of pestilence, the natives or long residents of the West-Indies are comparatively seldom subjected: the same causes producing, in persons habituated to their influence, the milder forms of remitting and intermitting fevers. Individual misconduct (as intemperance, &c.) occasionally excite, even in them, the worst kinds of fever; and, in particular seasons, persons of irregular lives, who reside in the midst of the accumulated causes of pestilential diseases, fall victims to their carelessness, in common with new-comers from more temperate regions. And such appear to have been the facts in Grenada in the year 1793.

Viewing the fever of that year as originating in local causes, rendered unusually active by the circumstances of season, &c. there will be no difficulty in accounting for its disappearance; without the encumbrance of the objections opposed to the doctrine of its being a disease of specific contagion. For,

I. There is reason to believe that a considerable part of the persons peculiarly exposed to the fever, had either died of it, or, having been sick, had recovered, and thus gained an immunity from a second attack for that year, and perhaps forever.

II. A large number of sailors, a description of men remarkably subject to the disease, were sent away from Grenada, about the last of August.

III. But the most material circumstance, probably, was the setting in of violent and long-continued rains; accompanied "with most vivid lightning and tremendous thunder, and violent squalls from the south-east." The rain which commenced so violently in September, continued through the greater part of October, almost all November, and the following December. Here, then, is discovered a cause adequate to the thorough purgation of an infected atmosphere; to the complete ventilation and cleansing of filthy streets and docks; and to the wholesome overflowing of a pestilential marsh.

It will avail the advocates of importation very little to remark, that the course of similar fevers in the United States, finds its only effectual enemy in cold and frosts. These obstacles can not operate in the West-Indies, as they do not exist there—at least, not at St. George's in Grenada, where the mean height of the thermometer is about 80, during the whole winter. Other causes of the discontinuance of pestilential diseases in that climate, must be sought after; and there can be little doubt but that those above-assigned, were in fact, operative and successful in the present instance; especially when it is remembered that the fever of Grenada wholly disappeared in the course of the tempestuous month of September, 1793. And hence a new argument against the doctrine of importation may be derived, as the difference observable in the operation of similar diseases in different countries, shews how far even the morbid seeds of pestilence are adapted to the climate in which they are destined to be sown and to vegetate. If there were not some variety of this nature, why should not the same cause arrest its progress and, effect its banishment, in the United States, that is adequate to these beneficial purposes in the West-Indies? Nor is the supposition any objection to the moral dispositions every where manifested in the universe. Vice and suffering constantly preserve an intimate connection. Their severance would be alike the signal for the overthrow of all virtue, and the demolition of every hope of happiness. The sources of pain,

no more than the occasions of vice, are limited to a particular climate and country; and they who wantonly indulge in intemperance, and obstinately neglect to remove the causes of disease, have no reason to complain of the visitations of gout, stone, dropsy, and pestilence. With the amendment of their own lives, and the correction of their own negligence, their miseries and their danger will disappear. Let them fulfil the injunctions of reason, and they will find no cause to inveigh against the injustice of heaven.

The principal reasons have been now displayed for discrediting the assertion of Dr. Chisholm relative to the introduction of the Grenada Fever, into that island, from Bulama, by the Hankey. A few remain to be disclosed, in an attempt to discover the motives of this author, for a misrepresentation so atrocious. These will be stated as briefly as possible. It may be proper, first, to remark, that none of the people who came to Grenada, in the Hankey, were sick there, at all, except Capt. Cox, whose intermitting fever did not entirely take leave of him till after his final departure for Europe.*

It will be remembered that the expedition to Bulama was set on foot by an Association of philanthropic gentlemen in England, with an express design of counteracting, as far as possible, the iniquitous traffic in human flesh; and, as a principal means of effecting this purpose, for the culture of those commodities which are the prime articles of exportation from the West-Indies, more especially of cotton. It will naturally be supposed that an enterprise like this would be regarded with evil eyes, by the West-India planters; to whose opposition and intrigues the shameful delay of justice in the British Parliament, is chiefly to be attributed. With what temper Dr. Chisholm regarded this effort of the friends of man; by how different principles he was influenced; or, at least, how ready he was to sacrifice truth at the shrine of prejudice and the wishes of his West-India patrons; is evident from his own words, when, speaking of the motives which determined the colonists to embark for Bulama, he describes them as "induced by "the delusive prospect of wealth held out to them," (an indirect charge on the benevolent projectors,) "and the fanatic enthusiasm "for the Abolition of the Slave Trade, &c." Who can doubt of the perversion of his understanding, or the pliancy of his faith, that recollects Dr. Chisholm's *voluntary* declaration to Mr. Paiba, that he was convinced of the local origin of the Grenada Fever,

* He arrived at Grenada well; but from subsequent inattention, and perhaps irregularity in returning too hastily to the indulgences of healthy men, suffered frequent relapses, or returns of the ague; but was at no time dangerously ill.

and that a similar disease had prevailed there, a few years before, from similar causes? Is it possible to credit the assertions of such a man on a point like this, when he is proved to be guilty of such palpable tergiversation, and when the violent and atrocious measures employed by the Grenadians are recollected and considered? What they were, and how successfully they were renewed and seconded in England, will be evident from the following extract of a Report from the Trustees of the Bulama Association, to a meeting of the subscribers, which the reader will find by referring to p. 159 of Part II. of an "Essay on Colonization, &c." By C. B. Wadstrom, published in 4to. at London, in 1794.*

"Having thus stated," say the Trustees in their report, "the proceedings at Bulama, to the 16th of March, 1793, it is necessary to remind the Association that, about that time the subscribers here had several meetings, to consider what could be done for their friends at Bulama, for whose safety the want of information had excited serious apprehension.

"About the same time, the Hankey, Capt. Cox, arriving from Grenada, with sugar, cotton, &c. an extravagant, unfounded, and malicious rumour was raised, that this ship had brought the plague from Bulama to Grenada, and thence to England. It might easily have been ascertained, that the mortality at Grenada, while the Hankey was there, was owing to one of those disorders to which the West-Indian islands are unfortunately subject. But, so flaming was the zeal of the Grenadians against the Abolition of the Slave-Trade, and the free colony at Bulama, that they employed every illiberal art to prevent Capt. Cox from getting a cargo at Grenada. But their malice did not end here: they sent home representations, or rather misrepresentations, respecting his ship, which were so strenuously supported by the West-Indians, that Government thought it proper to put the Hankey under quarantine, and to have her cargo unloaded and examined, before it was brought up the river. *Every examination proved the falshood and malignity of the report.* Yet several months elapsed, before the owners could unload their ship. Lightermen, wharfingers, and lumpers, all had caught the West-India Fever: all believed, or pretended to believe, the imposture. Grave citizens and official men, were not ashamed to countenance the wild opinion. Magistrates and members of the Corporation of London, were applied to, for

* It should be remembered that this is not a vague accusation from a few obscure individuals, but that the Report above mentioned, comes from men of the first character in Great-Britain, for probity, talents and information; from men whose situations and inclinations alike enabled and disposed them to obtain the most decisive evidence as to the facts they publish—their names have already been inserted at page 473.

“ their influence with government, to avert the impending pestilence. And, so successfully was the deception managed, that, on the famous 9th of November, a citizen of the greatest respectability, then master of one of the first city companies, forgetting that his sole province on that day was to dispense mirth and good cheer, sent to the lord mayor and aldermen, then on board the city barge, a representation of the alarming consequences that would ensue, if this terrible ship and her cargo were not destroyed! the alarmists gravely asserted that *all the Hankey's crew had died, and that the Captain was dying*. It is true, that Capt. Coxe had almost died with grief, from the report cruelly circulated respecting his ship. But it is equally certain, that *not one of the crew had died*, either on the passage from Grenada, or in the river. The calumniators, however, had in view another object than truth; and their success was such as might gratify the most malignant spirits; for they had nearly caused the death of a man, who had never offended them, and materially hurt the property of the ship's owners, and, what must have been still more pleasing to *them*, greatly injured the Bulama Association.

“ The prejudices thus industriously raised against the colony at Bulama, and which, from the want of information, it was (at that time) impossible to repel, deprived the trustees of all hopes of success in *then* applying for a charter. They preferred waiting, till the torrent of malicious, or ignorant clamour had spent itself, and till time and undoubted intelligence should confirm, or contradict, the reports. The prejudices against Bulama, rendered it extremely difficult, if not impossible, to succour a colony, *then deemed pestilential*; and there can be no doubt that this was one great object of the calumniators.”*

IT is now time to close this Essay, which has already extended far beyond the writer's first expectations. The mass of evidence here collected, to disprove the authenticity of Dr. Chisholm's account of the origin of the Pestilential Fever of Grenada, in 1793, is such as may fairly be considered as conclusive. It is not, indeed, to be expected that the supporters of the theory of imported contagion will relinquish their general doctrine, though they should fail of deriving any countenance from what occurred in Grenada. The notion of importation has been too long, and too confidently maintained, to be abandoned while a single colourable pretext in its favour, or a single subterfuge remains. But, it may be remarked, in concluding, that there is great reason to believe that a well-conducted inquiry into every other pretended instance

* The passages in Italics, are so marked in the original.

of imported contagion would terminate, equally, in satisfactory evidence of local origin; or, at least, would discover abundant causes for hesitation and doubt as to its foreign derivation.

SINCE writing the preceding Essay, I have again looked over a paper, by Dr. Chisholm, published in the Edinburgh Medical Commentaries for 1793, and intituled "History of an Uncommon Epidemic Fever, observed in the Island of Grenada, &c."

This disease, which resembled in many striking particulars, the Yellow Fever of the United States, which it differed from in others, is attributed by the author of the paper to *marsh miasmata*, or more properly to *local causes*; and there is every reason in the world for believing this opinion to be correct. The date of the occurrence of this fever is September, 1791. It appears to have ceased about the first of January 1792.

The symptoms which resemble those of the Yellow Fever, are the inflammation and protrusion of the eyes, the intolerable head-ach, delirium, coma, yellowness of the skin, rawness and burning in the throat and stomach, and dark stools. Other general similarities are observable.

The effects of various remedies constitute another strong analogy between this Grenada Fever of 1791, and our fevers, and the Grenada Fever of 1793. Bark generally aggravated the symptoms; bleeding gave relief, and the blood was sily; and mercury, used to salivation, was a certain cure.

When these additional facts are taken into view, and considered in connection with what has gone before; with the extensive examination which the subject has undergone in America; and with the facts and reasonings advanced by Mr. Webster, in his letters to Dr. Currie, relative to *general contagion*, and which will receive additional confirmation in his Treatise soon to be made public; it would seem impossible for any mind, however subjugated and bigotted to former opinions, to resist the force of this accumulated weight of evidence. And it certainly deserves to be regarded as a very singular fact, that a man who seems to have observed and thought so correctly as Dr. Chisholm did in 1791, and who held such opinions as he then professed, should have taken ground so very different in 1793, and with so little shew of reason to justify the change. As the Medical Commentaries have a general circulation in the United States, it is to be desired, that those gentlemen who feel themselves interested in the present inquiry, should not neglect to compare the paper of Dr. Chisholm, here referred to, with his treatise on the fever of 1793. It can scarcely happen that they do not find themselves confirmed in the belief of the local origin of the diseases of both years.

March, 1798.

ARTICLE IV.

Some Account of a PESTILENTIAL FEVER, which prevailed in the Island of Jamaica, in the years 1793, 1794, and 1795. Extracted from a Paper presented to a Medical Society in that island, by Dr. JAMES WALKER.

THE Paper from which this Extract is made, appears to have been read by the author, to a Medical Society at Port Royal; which society had then lately been instituted, and originally for the purpose of observing the progress, studying the nature, and perfecting the cure of the disease to which it relates.

The author ascribes the origin of the disease to a ship from Grenada, which is supposed to have introduced it into Port Royal. Of this, however, he offers no evidence; and subsequent remarks would induce a conjecture that he held a different opinion.

The first attack was sudden, and without any previous indisposition of the person seized. It began with violent head-ach, and a remarkable throbbing of the arteries; frequently subsiding within an hour, and succeeded by apparent inanition, a low, quick, and feeble pulse, and shining, watery, or glassy eyes, almost destitute of expression. Irritability of the stomach was a pathognomonic sign. At first, nothing was ejected but the natural contents of the stomach; after repeated efforts and much straining, mucus and bile were thrown up; and afterwards, generally about the third day, a matter resembling coffee-grounds, and sometimes of the colour, consistence, and tenacity of tar. Hemorrhagies were frequent, from the mouth and nose, and sometimes from the axilla, anus, and vagina. The respiration was laborious; the languor and debility great; and the fever neither intermitted nor remitted. Near the close of the disease, in those cases which terminated fatally, a yellowness appeared first in the eyes and on the neck, gradually extending over the whole body, and acquiring a darker hue. Very few had petechial eruptions.

The character of this fever underwent some alteration in the year 1795. In 1793 and 1794, it never intermitted, and its duration seldom exceeded five or six days, but it more commonly terminated in two or three. In 1795, it was sometimes protracted to the sixteenth and twentieth day, and generally to the ninth and tenth. At first, it was confined to newly-arrived Europeans, such as seamen, soldiers, and passengers; but was said to have affected some of the old residents of the interior of the island in 1795. Of

this, however, the author had no personal knowledge; and appears to doubt, as he thinks the disease not communicable from one person to another; in support of which opinion he remarks, that "in the public Hospital, where many people were necessarily in the same wards with numbers in this fever, neither any of them, nor of the attendants upon them, were infected."

Several dissections were made by the author. Nothing was observed in the brain; further than that it appeared less full of blood than is sometimes seen. The pleura, peritonæum, lungs, pericardium, and intestines, were perfectly natural. No signs of fulness or inflammation on the liver, except a slight redness on a small portion of the membranous covering of the convex side. A slight appearance of inflammation was observable on the villous coat of the stomach. The gall-bladders were distended with thick black bile, of the consistence of tar, and some of the same substance was found in the stomach. The hardness and tenacity of this black matter was increased by the addition of a solution of volatile alkali (ammonia). Vegetable acid, poured on another portion of the same tarlike substance, excited considerable effervescence, which continued some time; after which the mixture assumed the colour and consistence of healthy bile. Whence the author infers, "that the acrimony of the disease, or the morbid bile, has an alkalescent tendency."

As a preventative, beside temperance and cleanliness, the author recommends the exhibition of mercurials; and seems to think that they may be advantageously continued even to the producing of a gentle salivation. He relates one fact, as connected with this point, which, as it is curious and probably little known, may properly be given at length, and in his own words. "A circumstance, worthy of notice, happened last war, at the taking of Fort Omoa on the Spanish Main; which Dr. Clark, then surgeon to the Pomona frigate, on that service, communicated to me. It may be in the recollection of most of us, that a malignant fever broke out there, with the type of a marsh fever, which swept away great numbers of men from all the squadron, but was also so fatal on shore, that the 79th regiment alone lost eight officers. Among the ships which were captured in that harbour, one was loaded with quicksilver, for the use of the mines; and the vessels which contained it being broken by the shot of the Pomona, the mercury was found floating in large quantities, and a number of men were sent on board to collect it; which they did with their hands, by throwing it into pails or buckets. These men were continued in this employ, during all the time they remained there; and not a man of them had the smallest complaint, though surrounded by disease and death."

The method of cure generally practised at first, in 1793, con-

sisted of bleeding, cooling laxatives, and antimonial in small and frequent doses; and, as the pulse was observed to sink very fast, recourse was had to blisters, and the bark. Blisters, the author thinks, often relieved the anxiety at the precordia; which was also sometimes removed by opiates. Every preparation of bark was rejected; on which account they gave it by injection; and as the signs of debility fast increased, administered snake-root, camphor, brandy, and other stimulants, with elixir vitriol, subacid drinks, &c. &c. This plan of cure was very unsuccessful. Bleeding increased the debility; emetics excited an irritation of the stomach, which nothing would allay; and it became necessary to have recourse to some more efficient aid. This, happily, was discovered in mercury; and as the result of very extensive practice in this disease, and with this remedy, the author lays down, as the most efficacious, the following method of cure.

1. Calomel is to be given in pills, or what is better suspended in mucilage, [that of the Maranta or Indian Arrow-root is particularly recommended], at such intervals, and in such quantities, as the exigency of the case may require; observing that, as the object of the physician is to procure ptyalism as soon as possible, it must be vigorously pushed. To render this the more certain, the same medicine must be rubbed into the gums and lips, and mercurial ointment on the arms, legs, &c. The throat of the patient must be kept warm with flannels; he must inhale the steams of hot water—irritate the fauces with stimulating gargles, or with masticatories of arum, pyrethrum, &c. &c. If by any, or all, of these means a salivation is excited, we are told that there are few instances of the disease proving fatal; and not one has occurred to the author. More commonly, the fever immediately disappears altogether, or very much abates, and yields easily to ordinary means.

2. The pain of the head is to be relieved by rubbing it with vitriolic æther, or sliced limes and salt.

3. The bowels are to be kept open by means of clysters.

4. The mildest foods and drinks are to be given.

5. The bark may be administered, after the disappearance or great abatement of the symptoms, in such form as is most agreeable to the patient.

6. The inconvenient ptyalism which sometimes continues long after the fever is subdued, Dr. Walker thinks is more certainly obviated by blisters to the back, than by the means customarily employed.

The following brief notices of cases in which mercury was used will give the reader a more complete knowledge of the author's practice.

1. Capt. Stobo took 304 grains of calomel, and rubbed in 31

of mercurial ointment, which produced a ptyalism next day, and he recovered.

2. — Roberts took 305 grains, rubbed into his gums and lips 3 ii, and upon his thighs and groins 3 vi of strong mercurial ointment, containing one third mercury: in five days was salivated, and recovered.

3. — Kinnear took 525 grains, rubbed upon his gums and lips 600, and 3 x of ointment on his body, in eight days: took acidulated bark afterwards, and was soon perfectly well.

4. Mr. Sorely. This case terminated unfortunately. He took between 13 and 1400 grains of calomel, and 3 vi of the ointment were rubbed in; he was also fumigated, and took some small doses of turpethum minerale; but without producing salivation, as was desired. "During this patient's illness, which continued nine days, he had scarcely any fever, and no symptom of dissolved blood, only about the seventh day a few drops of blood came from his nose, which his nurse attributed to his picking it. He shewed a little yellow tinge upon his eyes only the day before he died, and gradually sunk without a complaint, tho' perfectly sensible. He only said that his gums were a little sore; but it is to be observed that he had a diarrhoea the first three days, which probably retarded the ptyalism; but it was afterwards necessary to keep his body open, daily, by emollient laxative clysters." Afterwards Dr. Walker observes of mercury, "I have known it cure a dysentery, but never knew it produce even a diarrhoea."

Other instances are noticed of the successful use of calomel in very extraordinary doses: and one case is referred to, in which a gentleman of the name of Forbes exhibited 1600 grains to a single patient, and produced a cure.

Such are the principal matters in Dr. Walker's paper, which appear deserving the attention of practitioners in the United States. On the whole, whatever may be thought of the method of cure proposed by this gentleman, in pestilential fevers, one fact seems to be clearly ascertained, that calomel and mercurials in general, may be administered in much greater quantities than we have hitherto been accustomed to prescribe them, and if not with advantage, at least without injury.

ARTICLE V.

A Letter to Dr. — — —, in Answer to his Queries respecting the Introduction of the Mercurial Practice in the Vicinity of Boston, Massachusetts.

By EDWARD A. HOLYOKE, M. D. of Salem, Massachusetts.

DEAR SIR,

WHEN, upon reading some late English publications, you find the exhibition of mercurial medicines in inflammatory diseases recommended as a *new* practice, though the same is so common and frequent in this vicinity; you naturally inquire how long this practice has been in vogue among us, and by whom, or by what means, it was first introduced?

I know not whether I shall be able to make you any very satisfactory answers to these queries: I will however endeavour to give you all the information I am possessed of.

A physician from Scotland, who, as I have heard, was a disciple of the celebrated Pitcairn, and who was an intimate acquaintance of some of the first practitioners in Boston, and its neighbourhood, about 60 or 70 years ago, was much in the habit of administering mercurials, and, as I have heard, much promoted their use among us, if he did not originate it.

This practice was much promoted, too, by the writings of Dr. Cheyne, then, and for some time after, much read by physicians here.

But what probably most contributed to give the faculty a high idea of this medicine, and to bring it acquainted with its virtues and uses, was the happy effect it was found to have, in checking the progress of a most formidable disease, which broke out in this part of America about the year 1734 or 1735, and made cruel havoc, sweeping off multitudes of children, wherever its baleful influence extended: I mean the disease at that time called *the throat distemper*; which I suppose to have been of the same genus with Dr. Huxham's *malignant ulcerous sore throat*, tho' it was, I believe, much more frequently and rapidly fatal then, than it has appeared of late years among us, or than it has been at any time in Europe. No remedies, we are told, were for some time of any avail, to stop its career, and almost all who sickened, died. At length recourse was had to mercurials, as *typhethum minerale* and *calomel*,

and by these, aided by antiseptics, &c. physicians were enabled to make some successful opposition to its ravages.*

It was natural to extend the use of so efficacious a remedy to other disorders, and being found or thought useful in many other cases, it became accordingly much employed.

But at what period, or by whomsoever the mercurial practice might have been introduced, in this part of the country, this is certain, that upwards of 45 years ago, it was in common use, in pleurisies, quinsies, inflammatory rheumatisms, and other phlegmasiæ; with several gentlemen who were at that time of the first repute as physicians. And this practice was not only adopted by their pupils, but by many other practitioners in the vicinity, and has not, since that time, been wholly laid aside, though I believe it has not been so much in vogue lately, as it was from 30 to 45 years ago. The modern European medical writers, who are most consulted and followed, by the faculty here, being totally silent with respect to the exhibition of mercury in fever and inflammatory diathesis, has, I doubt not, been the occasion of its running into disuse of late. The practice has, however, been still kept up by many, and will doubtless go on increasing, now European writers give it their sanction.

An idea that mercurials were improper, if not injurious medicines, in inflammatory cases in general, seems to have been adopted by physicians in Europe;† but certainly without just foundation, if the above account deserves credit; or if we may believe several European performances lately published; particularly a paper written by Dr. Wright, and inserted in the 7th volume of *Medical Facts*, entitled, *Practical Observations on the Treatment of acute*

* I remember to have heard a little anecdote, which may be worth relating on this occasion.—A practitioner in a neighbouring town, of great repute and extensive practice, being called to attend a young woman dangerously ill of this distemper; having ordered her, among other things, four or five grains of calomel, was astonished the next day to find her relieved, greatly beyond his expectations. Upon inquiring of his pupil, to whom he had given his directions, whether his prescription had been followed; he found that his patient had taken 30 grains of calomel, instead of four or five: to which mistake he attributed the cure. From this time forward, in very dangerous cases, he used the medicine in much larger doses than before.

† I well remember, that, about the year 175—, Dr. Charles Russel, a young physician, (who had been pupil to a gentleman who employed mercurials in his practice very freely) then lately returned from London, where he had been some time attending at a public Hospital (Guy's or St. Thomas's), informed me, that upon his relating to the medical gentlemen there, the common practice in this part of America, of administering mercurials, particularly calomel, in inflammatory disorders, that they expressed great surprize at the account, and told him they should have apprehended, the most fatal consequences from such a practice.

Diseases, &c. The encomiums Dr. Wright bestows upon the administration of mercury, in a variety of acute cases, so well accords with our long experience of its efficacy and safety, in this country, that every practitioner amongst us, who has been in the use of it, will readily accede to them.

For my own part, I profess myself to have been in the habit of prescribing this mineral ever since the year 1751 or 1752. About that time, pleurisies and peripneumonies were remarkably prevalent, and might be called epidemical: the practitioners of this place made free use of it at that time, and, as we found its effects beneficial, have continued to employ it in similar cases ever since.

It is not pretended, however, that this practice is universally successful, or that it is admissible in all subjects: some persons, as experience shows, cannot bear mercury; a great degree of debility, and irritability, being the immediate consequence of its exhibition, even when given in very moderate doses. Others, from great tenderness and irritability of bowels, seem incapable of admitting a quantity of the medicine sufficient to affect the system. And others, from a certain peculiarity of constitution, though the bowels bear it well, are but little apt to be affected by it, although it be taken freely, and for a considerable length of time. But so far as my recollection serves me, I have never known a failure in pneumonia, where the patient began to take it early, could bear it well, the mouth became sore, and a gentle pyalism came on in a few days.

The preparation of mercury most commonly made use of was *mercurius dulcis*, or calomel; in larger doses joined with some purgative, when designed to act as a cathartic; and in smaller doses, of one or two grains, as an alterant, or when the intention was to affect the system: and then it was frequently combined with *camphor*, and sometimes with some preparation of *antimony*, and sometimes with small doses of *opium*; or with all of them together, as the prescriber judged most proper: though, in some cases, the native mercury, rubbed down with Terebinth, &c. was preferred.

Beside these, the *turpethum minerale* was often given in a few grains (from one to four), with a little *ipecac.* as an emetic; than which the *Materia Medica* does not, perhaps, afford one more certain or more efficacious; especially in inflammatory quinsies, the croup, or generally when tenacious phlegm or pituit abounds in the stomach. Small doses, too, of this last preparation, as one third, or half a grain, given in a little cons. rosar. or honey, and repeated at short intervals, as two or three hours, have been found to be most powerfully expectorant, in pneumony, where the lungs have been greatly obstructed and loaded with viscid phlegm: and I have seen a number of instances, where patients who seemed on

the point of suffocation, were snatched from the jaws of death, by a few doses of this medicine.

My intention in this letter, however, you are sensible, is not to enter into the mode of exhibiting mercurials, much less to treat of any particular disease; my design is merely to answer your queries; to corroborate Dr. Wright's practice, by showing how it corresponds with a practice that has long been common among us here; and to show, that, in this part of the country at least, the same medicine has been successfully employed, certainly for nearly half a century, and probably much longer.

I am, &c.

E. A. HOLYOKE.

Salem, December, 1797.



ARTICLE VI.

An Account of the PESTILENTIAL FEVER which prevailed at Newbury-Port, State of Massachusetts, in 1796; in a Letter to Mr. Smith.

By Dr. CHARLES COFFIN.

SIR,

IT gives me pleasure to find the plan proposed in your Address, undertaken in this country; and I hope you will be able to prosecute it with success. I should be happy to contribute in any degree to your assistance.

Respecting the origin of the disease, which raged here in 1796, there have been but two opinions. Some have supposed it was generated here; others, that it was imported from the West-Indies, in a vessel which arrived in May. Having never had an opportunity, till very lately, of conversing, as I wished, with the Captain himself, I was not prepared to state all the facts with certainty relative to the suspected vessel. The Captain's account is, that about twelve days before his arrival here, two of his men died on board of a putrid fever. After which he immediately cleansed the vessel, by washing it with vinegar, smoking it with tar, and dubbing the cabin and steerage floors. He threw over the clothes, which the sick had worn, and their bedding. Their other clothes were locked up in their chests, and afterwards sent home to their friends, in a neighbouring town, without communicating any infection. The rest of the crew escaped the disease. Here it may be remarked, that if the vessel, on her arrival, had been stationed at some other part of the harbour, it would have been conspicuous, whether it brought the fever or not. But not far from the wharf at which it was unloaded, a great quantity of fish had been dressed for the West-Indies; and the entrails left exposed to the air. The weather being uncommonly moist and warm, the exhalation was very offensive to the neighbourhood. In one of the houses nearest to the fish offal, the three first persons were seized with the disease; and within twenty or thirty rods the greater number of its victims lived. The majority of those who recovered, lived in other parts of the town. Most, if not all, who had it at a distance, had frequented the infected neighbourhood; but did not communicate the infection to their attendants. These are the principal facts, from which the fever's origin must be determined.

The symptoms were various. Most were seized with violent pains in the head, loins, stomach, and abdomen; great oppression upon the præcordia; nausea and vomiting porraceous matter; great sense of cold, especially in the extremities; and a burning heat in the intestinal canal, followed with loss of strength. Others complained more of pains in their arms and legs, with a general lassitude, and but slight sense of chilliness or oppression upon the præcordia. The pulse in the robust was, at the onset, somewhat hard and full, but in most cases small and weak, especially in the advance of the disease. The tongue at first was generally covered with a thin, whitish fur, which afterwards became thick, dusky, and often black in the middle. The skin, in a few instances, almost retained its natural appearance, but generally became sallow and yellowish; and, in two or three cases, of an orange colour. An indisposition to sweat, and some stricture upon the skin, were common. The eyes were spiritless, yellowish and sometimes red. The stools, in some, at the beginning, were nearly of a natural colour and consistence, but more frequently yellow; and in the progress of the disease, generally changed to a bottle green, and sometimes to the colour and consistence of coffee grounds. The urine was reddish and turbid, and sometimes, of a greasy frothy appearance. The crassamentum of the blood taken away was loose, and the serum very yellow. In the two first days there were irregular exacerbations. On the third or fourth, the fever and other symptoms often abated, and the patient felt almost well. This mitigation was frequently fallacious, and followed with vomiting of black bilious matter, like coffee grounds, together with great soreness of stomach, restlessness, dejection, and delirium, which issued in death. Livid spots appeared on the arms of some, and the extremities of many were cold and livid, several hours before dissolution.

The method of treatment was regulated by the symptoms. When there was any hardness or fullness in the pulse at the beginning, bleeding was used with advantage, and sometimes repeated. Emetics and drastick purges were found detrimental. Manna and Glauber's salts, dissolved in tamarind water, were copiously given, and with great effect. The patient's strength seemed to be increased by their operation. Spirit. Nitr. Dul. Elix. Camph. Spt. Minder. and Haust. Sal. were used in the intervals. In cases of dejection, languor and cutaneous stricture, blisters upon the arms and nape of the neck were very serviceable. The cortex, when it produced no tightness in the breast, was beneficial. The diet was oatmeal, panado, ripe fruit, salop, &c. The drink was lemonade, tamarind water, almond emulsion, &c. Clean linen, and clear cool air, were very necessary. Sometimes the heat in the stomach and abdomen was so great, that linen cloths, dipped

in cold vinegar, and applied externally, as well as clysters of equal parts of cold vinegar and water, injected, were very refreshing.

Of those who had the fever, a full third recovered. Many others were slightly affected, who, by gentle purges, antiseptic diet, and pure air, escaped. According to the list of the Committee of Health, the number of those who died in town did not exceed forty; and if all those were added who took the disorder here, and died out of town, it would not exceed fifty. Some died on the fourth, fifth, sixth, and seventh days, and very few after. The committee were very careful, by removing nuisances, to promote purity of air in all parts of the town. The first appearance of the fever was on the 16th of June; the last instance of its fatality, on the 16th of October, and the last but one, on the 5th. As the coolness of the weather increased, its rage was mitigated, and gradually extinguished. It is worthy of remark, that the whole number of deaths, in the course of the year, was greater than usual.

The above particulars, I believe, Sir, may be relied on; and if they reach the object of your polite letters, it will afford pleasure to your humble servant,

CHARLES COFFIN.

Newbury-Port, Feb. 15, 1798.



OF
visit
ven d
fect l
I v
that i
symp
plaint
of cas
of its
witho
every
appar
ing no
ed, an
nutes
or thr
cry of
symp
about
counte

ARTICLE VII.

*Singular Cases of Disease in Infancy. Communicated in a Letter to
Doctor JAMES HAMILTON, Junior, of Edinburgh, by DAVID
HOSACK, M.D.*

New-York, February 20, 1798.

THE following cases of disease in infancy I have several times met with in practice, but do not recollect to have seen described by any writer upon the diseases of children. Underwood has given a sketch of a disease in infancy, proceeding from what he calls "an imperfect closure of the foramen ovale,"* which bears the greatest resemblance to it of any thing I have seen described: perhaps it may prove the same disease; but as Doctor Underwood has not described all the symptoms which characterise it, and as with him it has proved uniformly a fatal disease, I trust the following account of it, and the mode of treatment which I have found successful, will not be unacceptable.

CASE I.

ON the morning of the 30th October, 1797, I was sent for to visit a child of Nathaniel Pendleton, Esq. of this city: it was seven days old, of apparently good constitution, and had been in perfect health from its birth.

I was informed it had been suddenly taken ill in the night, and that it awoke with an unnatural screaming, which was the first symptom of its indisposition. Mrs. Pendleton considering its complaints to proceed from some disorder in the bowels, gave it a dose of *castor oil*, which operated freely, but without any abatement of its symptoms: a drop of *laudanum* was then administered, but without the smallest benefit: the child still continued screaming every few minutes, but after each fit it was perfectly quiet, and apparently free from pain; but would not take the breast. Finding no relief from the oil or opiate, Mrs. Pendleton became alarmed, and early in the morning I was called to visit it. In a few minutes after I had entered the room, it suddenly screamed out two or three times. I at once perceived that this was not the natural cry of the child, and took a seat with the view to examine its symptoms more particularly. The fits of screaming returned about every ten or fifteen minutes; immediately after each fit, the countenance appeared natural, both as to the complexion and

* See Diseases of Children, vol. i. p. 62.

composure of its features, and the pulses beat with their usual frequency; but in the course of about five or six minutes, respiration became entirely suspended; the eyes now assumed a vacant stare; the countenance changed its colour; instead of the natural florid hue, it became of a dark livid blueish colour: this change was first perceived about the upper lip and chin, and was apparent to every bystander: the pulses became less frequent, and very irregular: in about eight or ten minutes the blueness was extended over the whole face; the extremities underwent a similar change of colour, attended with some coldness: at this time the pulses were scarcely to be perceived: in a word, the child, at this moment, manifested every symptom of approaching death. In this critical state of things it again suddenly screamed, throwing out its arms and legs as in convulsions, and took two or three violent inspirations: after these efforts, respiration was again entirely suspended; but, notwithstanding, its circulation was again for a short time restored; the leaden colour of its countenance disappeared, and its natural complexion, for the moment, perfectly returned. These last favourable symptoms did not continue: as before, in a few minutes, respiration being entirely suspended, the countenance became livid and blue; the circulation became slower and very irregular, and the extremities lost their heat. Such was the condition of the child, until another fit of screaming restored the respiration for a moment, which gave new energy to the circulation. I was now satisfied that the bowels had no connection with its present complaints. Having seen the disease in three other children, I readily recognized it; but as, in all those, it proved fatal, I had not the smallest expectation of its recovery. In those cases evacuations were first employed, and afterwards anodyne medicines were administered, upon the principle that the disease was of a spasmodic nature; but as these remedies were not attended with any relief or advantage, I resolved to try the effect of a stimulant mode of treatment. Accordingly, I applied a small blister upon the breast, and two others behind the ears; cataplasms, composed of rye meal, vinegar and mustard, to the soles of the feet; bathed its whole body with warm brandy—afterwards, with diluted spirits of sal. ammoniac; and, from time to time, we attempted to give it a spoonful of wine whey, but to no purpose, as it was incapable of swallowing. This treatment was pursued for two or three hours: in the beginning it appeared to promise relief; the fits of screaming did not return so frequently; respiration continued a longer time, and the circulation became more vigorous. In about four hours the blisters produced their effect; upon their operation we had great reliance; but they produced no sensible alteration in the state of the disease: the fits still continued to return as in the beginning, and with the same succession of symptoms as above described.

fe
in
les
col

ba
eas
a t
po
two
sp
abo
nec
to t
I
took
perf
cour
ral e
resp
thing
nels,
retur
from
an ho
and v
circu
scribe
same
hour
appea
we w
screa
tacke
I can
of sho
formly
has ha

IN
Alexan
attende
of goo
immed
Vol.

Six hours had now elapsed; the cries of the child became more feeble; respiration was suspended for a greater length of time than in the commencement of the disease, and the circulation became less vigorous: these, with the greater coldness of the extremities, confirmed us in our fears of the event.

Having frequently employed a stimulating bath, prepared with bark and spirits, in the latter stages of malignant fevers, and diseases of great debility, I determined, as a last resource, to give it a trial in the present case. I therefore directed four ounces of powdered Peruvian bark to be boiled for a few minutes, in about two gallons of water; to this, when fit for use, a pint of Jamaica spirits was added: when it was cooled to a temperature rather above that of the body, the child was immersed in it up to its neck; and to render the bath more stimulating, I added, from time to time, a small quantity of the spirits of sal ammoniac.

In a very short time a favourable and very apparent change took place; respiration, while the child remained in the bath, was perfectly restored; the circulation became vigorous and active; the countenance resumed its lively hue; the eyes recovered their natural expression; the power of swallowing returned, and, in every respect, the child manifested symptoms of relief. In this state of things we removed it from the bath, wrapped it in warm dry flannels, and put it to the breast. To the great joy of its mother, it returned to it with eagerness; it remained composed, and free from complaints a considerable time, perhaps for the space of half an hour. At the end of that time, respiration became irregular, and was again interrupted, followed with the same change in the circulation and in the colour of the skin as has already been described. The bath was employed a second time, and with the same relief as before. It now fell into a sleep, remained so for an hour, breathing freely; its circulation regular, and the child, to all appearance, freed from its complaints. At the end of this time, we were again alarmed with the same distressing symptoms of screaming, and interrupted respiration, with which it was first attacked; but the bath as readily relieved it a third time. As far as I can recollect, it had two or three more paroxysms, but they were of shorter duration, returned after long intervals, and were uniformly relieved by the use of the bath. Since that time the child has had no return of the disease, and is at present in perfect health.

CASE II.

IN the month of December, a child of my brother, Doctor Alexander Hosack, was suddenly seized with the same disease, and attended with precisely similar symptoms: it was six weeks old, of good constitution, and had been remarkably healthy. I was immediately called to visit it, and upon entering the room, was

told that I had come too late, and that the child was dying. At this time respiration was completely suspended, the face of a dark blueish colour, the circulation weak, and very irregular, and the extremities were almost cold. I instantly directed a bath to be prepared in the same manner as I had done for Mr. Pendleton's child: in the mean time, the disease underwent nearly the same change and succession of symptoms as were described in the preceding case. When the bath was prepared, the child was immersed in it: in a short time, the respiration became regular: the child was continued in the bath about 15 minutes, but was by no means relaxed, as by the operation of the bath of warm water alone. When it was removed, it appeared to be perfectly relieved, and remained so for the space of twenty minutes, or half an hour; when the fit returned, but was of shorter continuance, and less violent: the bath was employed a second time, which completely removed all complaint. The child is now living, and in good health.

AS to the pathology of this disease, I dare scarcely offer a conjecture, having yet seen but five cases of it: this much, however, appears to me probable, that during the suspension of respiration, the blood ceases to circulate through the lungs, and passes by the former route through the *foramen ovale*. Hence we may account for the dark colour of the countenance, as occurred in the cases of obstruction in the *pulmonary artery* and *canalis arteriosus*, as related by Dr. William Hunter,* in the last volume of the London Medical Observations and Inquiries, which tends to shew the intimate connection between life, and the due oxygenation of the blood, by the process of respiration.

Why respiration should be thus suddenly suspended, becomes a question of importance, as, in the solution of it, the first cause of the disease is probably to be found: all the cases of it which I have seen, except the one last described, have occurred within the month; three of those within the first ten days after birth; and it is, perhaps, of importance to add, that the first three children in whom it proved fatal were of exceedingly delicate constitutions.

Another question relative to this subject occurs to me. What is the nature of the disease which so frequently proves fatal to children born at seven months? May it not proceed more especially from inability in the organs of respiration, by which that function is so readily interrupted, and consequently from a want of a due oxygenation of the blood? The symptoms preceding death are not unlike those I have described, and would appear to warrant the conjecture; but the cause which interrupts respiration in a child born at the full time, and of apparently good constitution, is not so apparent.

* See vol. vi.

 ARTICLE VIII.

 A CASE SIMILAR TO THOSE WHICH FORM THE
 SUBJECT OF THE PRECEDING ARTICLE.

By E. H. SMITH.

TO increase the scanty sum of information relative to a disease hitherto perhaps not very attentively considered, and imperfectly understood, is my inducement for annexing to Dr. Hosack's paper, the history of a case which is supposed by us both to be of the same kind with the cases related by him. Other practitioners, it is to be hoped, will supply those deficiencies, which, after all, will be no doubt observable both in the pathology and practice.

In the month of September, 1796, I was called to the infant child of Mr. Webb, of this city. The child was not more than nine days old; and had, till that time, been perfectly well. The pressing avocations of Dr. Moore, who was the family physician, obliged him to leave his patient under my sole care. The child had been repeatedly convulsed, before I saw it,—which was about noon—in the course of the day, and had taken laudanum and catmint tea, I believe by the direction of the nurse, without any relief, as the symptoms were afterwards much aggravated—perhaps in consequence. The convulsions recurring, after short intervals, and appearing to increase in violence every time, I ordered the child to be placed, up to the mouth, in warm water; in which it was held till the spasms disappeared, and the colour of the skin became more natural. This was repeated several times, without waiting for the recurrence of the fits; till the little patient grew easier, and I left it. The bowels were now, or had been previously, opened by an injection: so that there remained no reason for attributing any of the subsequent symptoms to irregularity of this sort.

In about an hour I was called back. The convulsions were more violent than before. The child was apparently too far exhausted to give any scream, or utter any sound; but the livor, or dark purple hue, came on about the mouth, thence extending over the face and whole body, and increasing in depth of dye as the spasms and difficulty of respiration increased,—and attended with the motions of the eyes common in convulsive cases. The intervals between the fits were now only of a few minutes; so that, though a change was observable in the colour of the skin, as the

convulsions grew less violent and the breathing more free, yet the skin did not resume a healthy colour.—I believe that I did not quit the patient for an instant, in more than four hours. In this time it had probably 12 or 14, perhaps 20 of these fits; the skin each time assuming this remarkable appearance, and in the manner above mentioned.—As it was easy to discover, by this change of colour, when the fit was returning, we had a bath of hot-water and ardent spirits at hand, into which the child was immediately placed; and where it was continued till the symptoms mitigated, or intermitted.—At length, on the recurrence of another fit, respiration was completely suspended: and neither pulse, nor motion, was perceivable. The skin, too, which during the whole, was preternaturally cool, now felt cold. In this state she remained, till a servant could run down stairs, procure me a quill, bring it to me, and allow me to prepare it for my purpose. This might be near a minute. Then, I inflated the lungs through one nostril—previously closing the other, and the mouth. This was done repeatedly: imitating natural breathing, by gently pressing the air out, and reinflating the lungs, till regular respiration recommenced.—Similar cessations of breathing, and, as it were, of life, occurred afterwards; and I was obliged to repeat this process several times, at intervals of from 15 to 20 minutes. At length, by persevering in the application of heat to the skin, by means of the bath, and of air to the lungs, in the manner described, the severity of the disease was overcome; the fits grew lighter and lighter, and the intervals longer and longer. The child fell into a natural sleep about sun-set; and the next day had no other complaint than debility; nor was any medicine afterwards administered.

It is important to observe, here, how much the preservation of life, in these cases, will depend on the presence of the physician. The attendants and nurse, of the child in question, were so persuaded of the inutility of all endeavours for its relief, that I am convinced that nothing but my presence and repeated solicitations, and even orders, could have prevailed on them to continue their efforts, or aid me in mine, for its restoration.

WITH respect to the nature of this disease, the result of my reflections on it is, that it must be ranked under the general name of *ASTHMA*, and with that particular species called *convulsive asthma*, by Dr. Darwin. I agree with Dr. Hosack in attributing the change in the colour of the skin to the deficiency of oxygen in the blood; to which deficiency, no doubt, the disagreeable symptoms are ascribable. But this change, and these symptoms, may take place, as in the case of drowned persons, &c. without supposing any unnatural circulation of the blood through the *foramen ovale*. That no such circulation actually happens, in

these
me
ble—
short
any
it to
time
such
tinue
woul
days
mina
hour
wood
moti
serva
wise
or al
gene
heart
throu
circu
once
any s
men o
duced
case.
Pend
Web
so co
tion
same
but v
I s
sugg
desc
fants
ferin
vanc
M

these cases, more than in health, [for we all know that this foramen is seldom perfectly closed in early infancy,] appears probable—1. From the sudden access of this disease—2. From its short duration—3. From its cure. For, 1. Were we to suppose any unnatural enlargement of the *foramen ovale*, we must suppose it to be connate; or, if not connate, yet remaining when at any time produced. 2. In this case, the symptoms consequent on such a condition of the heart, must appear from the birth, or continue from the time of the occurrence of this condition. They would not suddenly shew themselves at nine, or ten, or fourteen days, or six weeks after birth, without any apparent cause; and terminate fatally, or be completely obviated, in 24, 12, or perhaps six hours. 3. In this case we should expect, according to Underwood's description, that all the symptoms would be aggravated by motion; but this is not a fact observable in this disease, or if observable, only so in a slight degree. 4. The method of cure is likewise unfriendly to such a supposition. The stimulus of warmth, or alcohol, or ammoniac, or cantharides, to the skin, and of oxygen to the lungs, could have no effect on the structure of the heart. And though it may be said that the blood only passed through the *foramen ovale*, from inability of the lungs to admit its circulating through them, yet, it might be supposed, that having once reverted to its old course, and finding it so easy, it would, on any slight impediment in the lungs, be likely to recur to the *foramen ovale* a second time; and so the disease be a second time produced. But I have heard of no instance of this proving to be the case. It does not appear to have been in the children of Mr. Pendleton and Dr. A. Hosack; and it certainly has not in Mr. Webb's child; which was a fine healthy girl; and has ever since so continued. And I may add, that the same is the fact in relation to his eldest child, which was born in Dublin, where, on the same day from its birth, (the ninth) it suffered in the same way; but was relieved by means not known to me.

I shall dismiss this hasty consideration of the subject, with two suggestions. 1. The probability that the method of cure above described, would frequently succeed in recovering still-born infants. 2. That it may be worth observing whether persons, suffering the disease here treated of, are liable to the asthma in advanced life.

March, 1798.

ARTICLE IX.

An Attempt to accommodate the Disputes among the Chemists concerning Phlogiston. In a Letter from Dr. MITCHILL to Dr. PRIESTLEY, dated 14th Nov. 1797.

ON reviewing the state of philosophical controversy, as carried on both in Europe and America, between the phlogistians and their opponents, it has of late appeared to me, that much of the difficulty which attends the subject, arises, as in abundance of other cases, from the want of a precise language, and of a right understanding of each other's meaning. This was so evident to me, in the present case, that I informed my audience of it in one of my public lectures in Columbia College; and added my belief, that due attention to terms, their application and use, would have great influence in bringing the dispute to a termination.

Having subjected water heated to the temperature of steam in an eolipyle, and directed the stream issuing from it to the surface of red-hot charcoal, the coal brightened, and a greater flame was observed near the spot against which the steam was made to play. Here was an occurrence opposing the common observation of mankind, that water will always extinguish fire by reason of its own incombustibility. Water, kept at or below a certain temperature, will extinguish fire, and so will oil; but if water be raised to a heat sufficiently high, it will also burn, or undergo decomposition like oil. As far as I could judge from the phenomena before me, water, in proper circumstances, underwent a true combustion, and was inflammable, for the same reason that oil was, because it contained a something that would burn; and this something seemed to be exactly similar to that which made oil capable of exhibiting flame. It struck me instantly, that the inflammability of the vapour proceeding from burning fat, from heated alcohol, from camphor, coal, ether, and a multitude of other substances, gave evidence of their possessing a principle enabling them to burn with flame, after the same manner that water did. If there was this similarity, or indeed identity, of the inflammable radical among them, there appeared to be no more propriety in calling that radical *hydrogene*, than in terming it *olegene*, *alcohologene*, *etherigene*, *coaligene*, &c. To give the radical substance, enabling oil, alcohol, ether, and coal, to burn with flame, a name derived from water, because it enabled water to burn with blaze too, appeared to me partial, illogical, and wrong, inasmuch as it constantly and unnecessarily brought water and its properties to mind,

whenever any thing was thought of that contained hydrogen. And by this unhappy association, besides the difficulty which attends the subject in point of fact, vastly greater difficulty was made to surround it by reason of the ill-chosen and badly assorted terms employed in talking about it.

I had entertained no doubt, for two years, that hydrogen was an improper word for a nomenclature of science, and deserved to be struck out of the list: but as I was engaged in reforming another article of that arrangement, I chose not then to meddle with it. And I am glad I did not; since the prolonged disputes between the parties, afford more weighty causes for an alteration of terms at this day, than existed at any former time.

The circumstance common to all the processes I have mentioned, is "burning with flame or blaze," which, wherever it occurs, seems to indicate the presence of what has been called hydrogen. According to my present conception of the matter, this principle or substance, common to so many bodies, and enabling them to undergo inflammation, may, in strict propriety, be called *phlogiston*. I always thought *phlogiston* a well-conceived word, and have objected to it not on account of the impropriety of the term as such, but because of the vague and unsatisfactory way in which it was used. If a definite signification can be affixed to it, I think the adoption of it will be still a great acquisition to philosophical language, and have a tendency to settle at least half the controversy which divides the chemists.

I propose, then, to expunge hydrogen, and substitute *phlogiston* in its place. *Phlogiston* will thus be the radical term, and mean strictly the thing, in combustible bodies, which forms blaze or ignited vapour. The union of this with mere caloric, will make *phlogistous*, or inflammable air; the air which burns with blaze. The combination of *phlogiston* with oxygen, will constitute water, or the oxyd of *phlogiston*, one of the products of inflammation, and, like fixed air and other compounds, formed during the same process, incombustible in common temperatures and circumstances, afterwards. And the cause of this slowness to burn, of water and the other compounds which combustion furnishes, is owing to the large dose of oxygen with which they are charged, giving them little or no appetite for more. If the base be united to a yet larger quantity of oxygen, it will form the acid of *phlogiston*, or water soured by excess of oxygen, as, perhaps (though I do not believe it) in what is termed the pyro-lignic and pyro-mucic acids, and perhaps in some other cases; but the readiness with which *phlogiston* parts with its surplusage of oxygen, turns back to water, and preserves itself in that oxydated form, as proved by the operation of sharp pointed filaments under water, in effecting the separation, shews that Nature, in enabling the principle of inflamma-

bility to combine with oxygene, disqualified the latter, in most cases, from becoming an acid with the former; unless it should be found (and in this I have no faith) that the formation of the native acids of vegetables is a process of this kind. Should this latter conjecture turn out to be the fact, there would be instances enough of *phlogistic* and *phlogistous acids*.

On the decomposition of fat and oil by fire, it is known that a large quantity of water is formed, and this, probably, by an union of the base of vital air with phlogiston or hydrogene. The like obtains in the inflammation of alcohol, ether and coal, the phlogiston or hydrogene of which apparently turns to water, by junction with oxygene. And the principle, which, in the first instance, readily exhibits the blazing appearance, is changed, by and during that operation, to a something much more difficult to inflame, by any after-process.

If hydrogene, or phlogiston, is the material which inflames in the substances already mentioned, there is presumptive evidence, upon the face of the subject, of its existence, also, in the common sulphur, phosphorus, zinc, and iron, of the laboratories. I do not mean to say it is a *necessarily* constituent part of either of those bodies, for I believe they may exist without it, or at least, they may be conceived to exist abstractedly from it. They therefore stand very well in the nomenclature as simple substances. But if these substances, such as we commonly get them after exposure to the common atmosphere in ordinary temperatures, are taken for simple or pure elementary bodies, the persons who consider them so, fall into a great mistake. In their usual forms they are all incorporated with hydrogene or phlogiston, and from it derive their capacity to burn with flame. This will be the more clearly seen by considering them more particularly one by one.

OF SULPHUR.

I. The phlogistians say, sulphur is composed of phlogiston united to vitriolic acid; consequently, if any thing takes away that ingredient from the acid, this will turn to brimstone. The anti-phlogistians affirm sulphur to be a simple body, uncombined chemically with any thing; and that it becomes sulphuric acid by junction with oxygene. Now, both parties have reasoned in a manner that does not by any means satisfy me. They have viewed the combustion of sulphur in the abstract, rather than taken it up as it is. The fact is, that the acid formed in the combustion of sulphur, is not the solid crystallized matter, or the glacial oil of vitriol, but a solution of these crystals in water. The existence of vitriolic acid in a fluid form, implies necessarily the co-existence of water. The formation of the water in the inflammation of sul-

phur, appears to have been passed over by both parties; though the interpretation of this part of the process seems to me to furnish the means of reconciliation. Thus, while the pure sulphur combines with one portion of oxygene to make the acid, the hydrogen, or phlogiston, unites with another parcel of it to form the water in which the acid dissolves. Common brimstone, then, is not a simple substance, but is a *phlogisture of sulphur*. And this is confirmed by the fact, that where combustion is restrained, the sulphur may be resolved into hepatic gas; the phlogiston turning with caloric into inflammable air, and dissolving some of the sulphur. The formation of common hepatic gas seems to evince the same thing; for while the pot-ash seizes the sulphur, the hydrogen, or phlogiston, is set loose, turns with caloric to hepatic gas, and snatches, as it departs, a portion of the sulphur from the alkali. Thus it appears that the two systems are reconcilable with each other. When the old chemists talk of phlogiston, they should define it to be that thing which burns with flame, and, when united to oxygene, forms water. When the new ones make experiments on sulphur, they should remember that the common material called by that name, is not the abstract, pure, uncombined, elementary thing they intend in their nomenclature.

OF PHOSPHORUS.

2. In like manner the phenomena attending the inflammation of phosphorus seem to have been as negligently interpreted. Phlogiston, added to phosphoric acid, was said by some to constitute phosphorus; while oxygene, added to phosphorus, made phosphoric acid, in the opinion of others: but these were a kind of chemical theorems, true only in the abstract. Experimenters find that phosphorus burns *with flame*, and *water* is exhibited during the process. All that needs be said about it is, that in common circumstances phosphorus, though capable of existing per se, has a very strong attraction for hydrogen or phlogiston, and, in ordinary cases, attaches more or less of it to itself. During its inflammation, a part of the oxygene, as in the case of the sulphur, combines with the phlogiston into water, and another part of it joins the oxygene, to constitute the acid. In estimating the *whole* of the process, the candid partizans of both sides will allow, that the substance under consideration parts with its phlogiston, and borrows oxygene; and thus water, and the acid dissolved in it, are formed.

Where is the harm of owning that common phosphorus contains a portion of hydrogen, united with it? It does not invalidate the modern theory, but it shews that the objections of the ancient doctrine were not frivolous, as they have, by some, been

deemed to be; but, on the contrary, very substantial, and not capable of reconciliation, upon any other plan that I know, than the one herein suggested.

OF ZINC.

3. Zinc may be abstractedly considered as a simple body; and with propriety placed as such in the catalogue. Commonly, however, it is presented to us in close connection with hydrogen, for which its attraction is so strong that they commonly appear in the form of a *phlogisture of zinc*. When that composition is employed for experiments, it is very easy to conceive how, when such zinc is exposed to a sufficient heat in an open fire, the phlogiston dislodged, and immediately becoming phlogistic gas, or inflammable air, shall take upon itself the form of flame, and constitute water; while the oxygen combines with the metal into a white oxide, the flowers of zinc. So, if the same compound be dissolved in sulphuric acid, the phlogiston displaced will turn to inflammable air, while the acid and the zinc form white vitriol. In this way some of the phlogistic or inflammable gas may be accounted for, as extricated from the metallic preparation; and, at the same time, I see no objection to deriving the rest of the great quantity afforded by this process, from the decomposition of part of the water or oxide of phlogiston. To accommodate matters then, the advocates of the Lavoisierian theory should concede that zinc, in common circumstances, is associated with hydrogen or phlogiston. And the disciples of Stahl should, on their side, allow, that zinc cannot be considered as a pure metal, while alloyed or blended with phlogiston, or any other foreign ingredient. The material they have all worked upon is not the uncombined metal, but a *phlogisture of zinc*.

OF IRON.

4. Some sorts of iron treated by heat alone, afford phlogistic or inflammable air. The same metal may be made to burn with flame; and when treated with sulphuric acid, it affords much phlogistic air. What, then, is the thing commonly called iron? Is it a pure and unmixed substance? Or is it a compound of elementary iron with hydrogen or phlogiston? The facts enumerated lead conclusively to the latter opinion. The phlogistians are right, then, when they say common iron is a compound; and they are right when they say the inflammable air obtained from it is nearly pure phlogiston. And the antiphlogistians are justifiable in placing in their enumeration of simple bodies, such a thing as elementary

iron is or may be imagined to be; and in ascribing the production of hydrogenous or phlogistic gas to a decomposition of part of the water. The compound called iron, then, gives out something, and takes in something, in all the common processes. And the modern chemists should correct the mistake they appear generally to have fallen into, of taking it for granted, that that was a simple substance which in fact is a chemical composition of iron with hydrogen. And thus finery cinder, which evidently differs from hematites, or any pure oxyd of iron, may be a triple compound of iron, hydrogen, and oxygen, which just about corresponds with your idea, that it consists of iron and water.

It will not follow from all this, that because phlogiston or hydrogen so generally exists in combination with zinc and iron, that it must be an ingredient in all metals. For gold, arsenic, silver, platina, mercury, copper, tin, lead, bismuth, cobalt, antimony, manganese, are capable of existing without it, and accordingly do not commonly burn with flame, nor afford inflammable air by solution in acids; though, if ever they exhibit in any of their states, blaze by burning, or phlogistous air with acids, this will only evince the existence of hydrogen in them in such cases. Both parties may thus allow, that some metals contain phlogiston, and some do not.

Nor will charcoal, as has sometimes happened, be confounded with phlogiston, according to this view of the matter, though hydrogen is often blended with it. If coal, at any time, affords inflammable air, this is no evidence of the conversion of that substance into phlogistous gas, but merely a proof that the coal, when submitted to experiment, was combined with the basis of inflammable air, which it could part with and still remain coal; though, in that case, incapable of burning with flame: but in a sufficient exposure to heat and oxygenous air, taking on, without blaze, the form of carbonic acid gas. It may be conceded, then, on both sides, that though phlogiston or hydrogen may exist with coal, nevertheless coal can exist without phlogiston.

I know not how these things may impress your mind. They appear to me to go a good way towards reconciling the existing differences. If, however, I have in this deceived myself, I have only to appeal to your experience and candour. But at present I cannot help thinking that a part, at least, of the controversy between Mr. Kirwan and the French philosophers, as well as that between yourself and Messrs. Adet and Maclean, may thus be resolved. Nor do I see any thing in Mrs. Fulhame's experiments, which renders the accommodation more difficult.

Your opposition to the new doctrine has been serviceable to the cause of science. It has prevented too easy and sudden an acquiescence in the novel system of the antiphlogistians, whose difficul-

ties and paradoxes had been admitted by many, without having been subjected to due examination. You have prompted more vigorous inquiry into these matters than would probably otherwise have been made; at least, for myself, I acknowledge your views of things have induced me to examine the points under discussion, with much more attention than I should otherwise have done. Perhaps even now my labours are but of little avail; or, if they were capable of bringing about a coalition of parties, I might say to you, after all, in the words of Prior in his *Alma*:

For, Dick, if we could reconcile
Old Aristotle with Gassendus,
How many would admire our toil!
And yet how few would comprehend us!

I am, with many wishes for your happiness, and thanks for the publications you have from time to time favoured me with, sincerely yours,

SAMUEL L. MITCHILL.



A L

I

chen
opin
are c
is eit

Y

A cu
incre
be ex
steam
solva
the v
then
in th

M

exper
of be
after
his fi

As

merc
were
essent

Til
evid
shew
tains
well p
a reas
more

As

oblig
pamp
I am

ARTICLE X.

A Letter to Dr. MITCHILL, in reply to the preceding, by JOSEPH PRIESTLEY, LL. D. &c.

DEAR SIR,

I Thank you for your ingenious, and well intended, attempt to promote a peace between the present belligerent powers in chemistry; but I much fear your labour will be in vain. In my opinion there can be no compromise of the two systems. Metals are either necessarily simple or necessarily compound; and water is either resolvable into two kinds of air, or it is not.

Your argument to prove that it is, I do not think conclusive. A current of steam is accompanied with a current of *air*, which increases the fire; but cover the hot coal with steam, and it will be extinguished as soon as if it were dipped in cold water. The steam only supplies a basis for the air into which the coal is resolvable, carrying away both the phlogiston and the heat: whereas the vapour of oil is real fuel. When sent through a red hot earthen tube, it is converted into inflammable air; but water, treated in the same manner, is water still.

My father-in-law, who was an iron-master, was deceived by the experiment of the colipyle, and thought to save the usual expence of bellows to his furnace, by mounting a large one over it; but, after incurring much expence, this instrument soon extinguished his fire.

As to *metals*, all the calces of all of them that I tried, even that of mercury, imbibed great quantities of inflammable air when they were revived; so that I cannot but conclude that phlogiston is an essential ingredient in their composition.

Till the antiphlogistians can produce more unexceptionable evidence of the composition and decomposition of water, and shew that *finery cinder*, which they call *oxyde of iron*, really contains *oxygene*, the old doctrine will keep its ground; and, as I am well persuaded they cannot do this, I have no doubt but that, in a reasonable time, it will recover the ground it has lost, and be more firmly established than ever.

As I presume you are acquainted with Dr. Maclean, I shall be obliged to you if you will inform me when he replies to my last pamphlet. He did not treat me with the civility to which I think I am entitled as a veteran in the science. Had he been the victo-

rious Buonaparte, I am old Wurmser, and should have been treated with respect, though vanquished. But this Mantua has not surrendered yet.

I shall be very happy to hear from you again, and am, with much respect,

Dear Sir,

Yours sincerely,

J. PRIESTLEY.

Northumberland, January 18, 1798.

P. S. Dr. Maclean did not, as the laws of war require, ever send me a copy of his pamphlet; and as I never saw it advertised, it was only by the accident of my son's being in Philadelphia that I got it.



Som
B
E

Y
talit
and
Ass
of n
Thi
is un
but
our
thro
chee
place
of n
B
squa
runn
ritor
tion
and
the l
tlem
was
famil
famil
religi
the y
of th
conta
getati
the sp
pestil
most
says i
well v

 ARTICLE XI.

*Some Account of the Epidemics which have occurred in the Town of
BETHLEM, Connecticut; from its Settlement to the present Time.
Extracted from a Letter to Mr. Smith,*

By the Rev. AZEL BACKUS.

YOU have probably heard that the clergy of this State, in General Association, have agreed to keep an accurate bill of mortality, beginning January 1st, 1798, in their respective parishes and towns, comprehending age and disease. When the General Association meets in 1799, the public will be favoured with a bill of mortality, for the whole State of Connecticut, and so on yearly. This can be done with little trouble by our profession, and as it is understood that we keep a bill not only for our particular people, but of those of different denominations that happen to fall within our local precincts, it probably will be quite accurate. If I can throw in my mite for the physical happiness of mankind, I will cheerfully do it. To this end, I will give you a history of this place from its first settlement, as I have learnt it from the records of my predecessor, and the authentic information of the aged.

Bethlem lies south of Lichfield; and is between four and five miles square. Like the rest of Lichfield county, it is ridged into hills running due north and south. This little town contains in its territory, six of these undulations of hills, and between each undulation there is a perennial stream of water. The soil is very fertile, and at present very much cleared of its native wood. When the late census was taken it contained 1500 inhabitants. The settlement of the town began in 1730. The Rev. Joseph Bellamy was ordained their minister in 1740, there being then but 14 families. The popularity of their minister soon drew many other families from various parts; the forests fell; and it became a religious, a happy, a healthy, and prosperous community. In the year 1748, an individual built a dam across the central valley of the town, in order to kill the timber of a flat in the north part, containing about 50 acres. In this swamp, producing little vegetation, save black alders and poison sumach, a pond stood till the spring of 1750; when it was let out, and, with its stream, the pestilence issued, which carried off between thirty and forty of the most hale and robust inhabitants. Doctor Bellamy, in his records, says it was a nervous, long, but very malignant fever, and that the well were not sufficient to tend the sick. The old people tell me

that the neighbouring physicians called it the plague. Doctor Seth Bird informs me, that the symptoms of the diseased were a wire pulse, coma in a few hours after being taken, loss of reason, and most generally the loss of speech for twenty days in those that recovered; sores, the bigness of a dollar, on various parts of the body. — — — — —, whom you know, was supposed to be saved by pouring boiling liniment into these mortification sores on his body. The length of so malignant a disease, says the doctor, was something I could not reduce to theory; the faculty generally pronounced it *sui generis*.

As few of the physicians, and still fewer of the people, thought the ponded swamp the cause of the disease, ten years after what the old people call the first great sickness, the same low ground was again overflowed. After the letting out the water in 1760, another wasting sickness began in the month of November, and carried off about 40 inhabitants: this was called a malignant pleurisy. The sick generally died the fourth or fifth day; some in 24 hours. The whole town was afflicted with a hard influenza. It was in this sickness, that the story of the quails originated, which you have probably heard. A man of undoubted veracity was approaching a house in which five of the diseased lay, and saw a flock of quails fly over the chimney; five of the quails fell dead, which were, on the same day, seen by many persons still living, and by the credulous it was thought ominous; but of the fact there is no room to doubt. There is also a circumstance respecting this last disease a little peculiar and different from general theory. By the record of mortality it appears to have raged the most in the beginning of December. If you wish for more facts than what is contained in this outline, you will please to apply to Doctor Bird.

Very general health was enjoyed in this place, from 1761 to 1789, when the influenza had an universal run; in 1791, it was repeated with still greater severity. This could not be attributed to the old box of Pandora, the pond, which, for these twenty years, has been drained and reduced to a meadow. In 1792, we had five deaths among children, by the cynanche trachealis, vulgarly called bladder in the throat. In August of this year (that is 1792), there were seven or eight instances of the scarlatina, but not mortal. In February, 1793, the angina maligna attacked our healthiest children like an armed man. Nineteen, from the age of four to 20, fell victims to it in three months, but far the greater part of these in the month of March. The number who were sick with it I cannot tell. It was more or less in almost every family, and few of the children escaped a slight touch of it. The disease had every grade, from the slightest indisposition, up to dying in 12 and 20 hours from

the time they were seized. From the first of May it began gradually to decrease, until it totally disappeared in November.

On the 10th of January, 1794, the angina maligna, or canker rash, reappeared: between this and the 20th of May following, we lost 13 under 20 years of age, and one aged 25. Many who now had the disorder, had had it in 1793. It was not so generally prevalent as before, but quite as malignant. There was no instance where the disease terminated in swellings about the throat, but they recovered. Many of the instances where death ensued, seemed in the outset to bid defiance to medical skill. Life seemed to rush away *qua data porta*, and to mock at medicine.

If you wish for a more circumstantial account, you must inquire of our acting physicians. We have no certain evidence that it was contagious; but several instances of the disease in families where there had been no communication with the sick. I would observe, however, as I was often among it, I twice or three times had the sore throat, and had my tongue striped with it; and often felt sensations, while in rooms with the sick, like the slight stinging of bees about the passages of the throat. Something more than could be attributed to sympathy, or the powers of imagination.



ARTICLE XII.

SOME ACCOUNT OF THE COUNTRY AND CLIMATE
OF THE NORTH-WESTERN LAKES.

By Major C. SWAN, Paymaster to the Western Army.

NOTE. The following Article consists of Extracts from a Letter of Major SWAN, to Capt. FRYE, Commandant of the Garrison at Governor's Island, who was so obliging as to communicate them to one of the Editors; and they are now made public with the consent of the Author. These Extracts are briefly made from a Journal kept by Major SWAN, at the time; and are inserted for the threefold purpose of illustrating the table of thermometrical observations which follow, of communicating a short, but authentic view of a part of the North-Western Territory, and of inviting further information.


D'Etroit, October 10, 1797.

"THIS country is yet new, and almost in a state of nature, like its inhabitants. It is true, the soil is extremely rich and fertile; and it is to a superabundant burden of vegetation, and a flat surface for hundreds of miles together, producing much stagnant water, that we may attribute the unwholesomeness of the climate, which is almost certain to affect the inhabitants with bilious complaints every fall."

"General Wilkinson arrived here in June, this year; and, after making some prompt arrangements for the garrison, proposed a voyage to Michilimackinac, and invited me to accompany him; and on the 4th day of August, we embarked in a sloop of about 70 tons burden. We had a safe and pleasant trip, not only to

* These remarks have particular reference to the neighbourhood of D'Etroit.

Michilimackinac, but even into Lake Superior; and returned to this place, on the 4th of last month, highly gratified indeed.

"We first left this place, and traversed Lake Sinclair, a handsome circular lake, about twenty-five mile across. We then proceeded up the river of that name, which is broad and very handsome, for about forty miles, to a rapid at the entrance of Lake Huron; traversed this immense, beautiful lake, three hundred miles long; and arrived, on the 15th of August, at the streight which unites it to Lake Michigan. This streight is broad; and the Isle de Bois Blanc, or white-wood island, Round Island, and Michilimackinac Island, form a cluster in the middle of the streight, and afford a romantic and majestic landscape from the sea. The Isle de Bois Blanc is eleven miles and an half long, and from two to three wide, lying parallel to the two coasts of the streight, but nearest to the south side. Round Island is about three miles in circumference, and lies at the upper or south-west end of De Bois Blanc. The island of Michilimackinac is circular, and lies between the upper end of De Bois Blanc and the north-western coast of the streight; having a channel of about one mile and an half between it and De Bois Blanc, and a channel of nine miles between it and the north-western coast of the streight. It measures seven miles and three quarters in circumference, and is nearly circular. On the south side of this island, there is a small bason, of a segment of a circle, serving as an excellent harbour for vessels of any burden, and for canoes. Around this bason the village is built, having two streets of nearly a quarter of a mile in length, a Roman chapel, and containing eighty-nine houses and stores; some of them spacious and handsome, with white lime plaistering in front, which shews to great advantage from the sea. At one end, and in the rear of the town, is an elegant government-house, of immense size, and finished with great taste. It is in the form of ; one story high, the rooms fifteen feet and an half in the clear. It has a spacious garden in front, laid out with taste; and extending from the house, on a gentle declivity, to the water's edge. There are two natural limpid springs in the rear of the house, and a very lively grove of sugar-trees, called the park. Suitable out-houses, stables, and offices are added; and it is enriched on three sides, with beautiful distant prospects. Twenty rods from the rear, there is a sudden and almost perpendicular ascent of about a hundred feet of rock, upon the top of which stands the fort, built of stone and lime, with towers, bastions, &c. occupied by our troops, and commanded by Major Burbeck. About half a mile from the fort, in the rear, there is an eminence, which I estimate to be about two hundred and fifty feet from the surface of the water. This spot commands an extensive and sublime view of the adjacent country. The fort, the village, the

neighbouring islands, and channels, seem prostrated at your feet; while, to the south-west, you look into the immensity of Lake Michigan, which loses itself in the southern hemisphere; and, to the north-west, the great Lake Huron lies expanded to the bounds of the horizon. It was a beautiful morning when I had this view.

"This celebrated streight is the only key to the immense, lucrative skin-trade, now solely carried on by British subjects, from Montreal, with the nations of Indians called the Sauteurs or Chipewas, Sioux, Reynards, &c. who inhabit the water-courses that fall into the Mississippi between the Illinois and the Falls of St. Anthony. Canoes are loaded and fitted out by these traders every year, from Michilimackinac. They commonly set out in July, and return in June, July, or August, the year following, to Michilimackinac, from whence they started. Here they are again met by the Montreal canoes, with fresh goods, exchange loading, and each return from whence they came. The Montreal canoes penetrate to Michilimackinac, by way of Grand River; which, with the exception of a small portage, conveys them to the northern point of Lake Huron; and return by the same route. Those from Michilimackinac penetrate the interior or Indian country, by way of Green Bay, an arm of Lake Michigan; thence, through Fox River, into the Mississippi and its tributary streams; and return also to Michilimackinac by the same route.

"On the 22d of August we left Michilimackinac, and on the 23d anchored in the streight of St. Joseph, which leads to Lake Superior. At this place Nature has displayed very handsomely again. The mouth of the streight is about thirty miles wide, but so strewed over with innumerable small circular islands, that it is difficult to obtain a view in any direction of more than six or eight miles. Indians have sometimes been lost among these islands, for weeks together. They extend into Lake Huron, and continue along the north-west coast of the lake for an hundred and eighty miles, and are called, by the savages, the Meneto, or Devil's islands. From the entrance of the streight, at a place called the *Detour*, it is nine miles to the new British garrison, built on the point of the island of St. Joseph, commonly called the Carraboo island. This is the largest in the streight; being about twenty-five miles long, and from ten to three broad.

"On the 23d of August, we left the vessel, embarked in three canoes, ascended the streight in what is called the canoe channel, and encamped at Muskito Point.

"The 24th, at one o'clock P. M. we arrived at the Falls of St. Marie, called *le saut de St. Marie*. These falls are about three quarters of a mile long, and half a mile wide; the rapid not violent; and the perpendicular of the whole fall about thirty feet.

T
ing
occ
pan
*
*
from
At
into
a fi
the
the
laun
safe
"
and
vess
"
at su
"
taine
ever
Lake

O
with
ence
error
were
Islan
1798
was v
exper
1797
garde
part o

There is a small kind of village on the United States' side, containing sundry large ware-houses, and a few decent dwelling-houses, occupied by the Agents of the Canada North-west Trading Company. There is not a clear white-woman in the place.

* * * * * "The 25th it rained. * * * * *

* * * * * "On the 26th we set off, in two bark canoes, from the upper end of the portage, for Lake Superior. * * * * * At one o'clock P. M. we entered Lake Superior; looked fairly into it; drank of its waters; ate our dinner; and put about, with a fine fair wind. We reached the falls again at four o'clock in the afternoon; placed experienced guides, with strong paddles, in the bow and stern of each canoe; hoisted the fifteen stripes; and launched into the bosom of the cataract. In a moment, we were safe in the bason at the bottom of the falls!

"We embarked early on the 27th. Having a strong current and fair wind, we descended in the ship channel, and reached the vessel, at Carraboo Island, at nine o'clock in the evening.

"The 28th, we put to sea again; and on the 4th of September, at sun-down, reached this place.

"I inclose to you, herewith, degrees of heat which were ascertained by regular observation with Fahrenheit's Thermometer, every day; by which you will perceive that the temperature of the Lakes differs widely from that of the Atlantic country."

On comparing the Table of Observations, which is annexed, with observations made on the same days in this city, the difference will appear so remarkable, that the reader may suspect some error in the instrument made use of by Major SWAN: and such were my suspicions. But, crossing the East River, to Governor's Island, in company with that gentleman, on the 4th of March, 1798, he observed that the wind, which we then felt, and which was very brisk, resembled, in point of temperature, that which he experienced on Lake Huron on the 14th and 15th of August, 1797; and added, that on the same evening, a frost affected the gardens at Michilimackinac so severely as to destroy the greater part of the vegetables.

E. H. S.

TABLE

*Of the Degrees of Heat observed on FAHRENHEIT'S Thermometer,
from August 4th to September 4th, 1797.*

1797.	6 o'clock A. M.	12 o'clock M.	7 o'clock P. M.	Average	REMARKS, where.
<i>Aug.</i> 4.	66	63½	59	62¼	Lake Sinclair.
5.	59	66	63½	62¼	Do.
6.	55½	65	66	62	River Sinclair.
7.	60	67	67	64	Do.
8.	59	61	61	60	Do.
9.	57	62	52	57	Do.
10.	58	62	61	60	Do.
11.	63	59½	60	60	Lake Huron.
12.	57	60	61	59	Do.
13.	55	55½	55	55	Do.
14.	50	49	47	48	Do.
15.	45	66	54	55	Michilimackinac.
16.	52	70	53	58	Do.
17.	54	69	60	61	Do.
18.	52	67	62	60	Do.
19.	54	64	56	58	Do.
20.	53	64	58	58	Do.
21.	62	63	63	62	Do.
22.	54	61	59	58	Streights of St. Joseph.
23.	57	65	60½	62	Do.
24.	50	63	60½	57	Do.
25.	46	53	49	49	Falls of St. Marie.
26.	46	49	50	48	Lake Superior.
27.	50	56	49	51	Streights of St. Joseph.
28.	40	50	46	45	Lake Huron.
29.	51	54	57	54	Do.
30.	49	57	53	53	Do.
31.	50	56	49	51	Do.
<i>Sept.</i> 1.	48	57	47	50	Do.
2.	51	59	50	53	Do.
3.	49	58	50	52	River Sinclair.
4.	48	56	49	51	Lake Sinclair.

ART.
in
M.
An
Pro
and
AC
Cu
Sou
ed
from
Vic
Phi

IN c
be
establi
only b
the w
graded
every
be rest
upon t
rience,
useless
of ind
cism.
physic
nature,
dicine
or esta
some r
rafter,
others.
but the
sible to
author
" pract
" consi

R E V I E W.

ART. I. *A View of the Science of Life; on the Principles established in the Elements of Medicine of the late celebrated John Brown, M. D. with an Attempt to correct some important Errors of that Work. And Cases in Illustration, chiefly selected from the Records of their Practice, at the General Hospital at Calcutta. By William Yates and Charles Maclean. To which is subjoined, a Treatise on the Action of Mercury upon Living Bodies, and its Application for the Cure of Diseases of Indirect Debility. And a Dissertation on the Source of Epidemic and Pestilential Diseases; in which it is attempted to prove, by a numerous Induction of Facts, that they never arise from Contagion, but are always produced by certain States, or certain Vicissitudes of the Atmosphere. By Charles Maclean, of Calcutta. Philadelphia. Young. 1797. 8vo. pp. 232. ,*

IN considering the present imperfect state of Medicine, it must be matter of regret, that so much yet remains to be done in the establishment of general principles. This desirable work has not only been retarded by the intrinsic difficulties of the subject, and the want of exertion among physicians; but it has also been degraded by the many frivolous and visionary speculations, which every where abound in books of medicine. If theory, however, be restricted to the legitimate deductions of reasoning, grounded upon facts, corrected or confirmed by the results of sound experience, it will not be easy to prove it to be either pernicious or useless. To inveigh against theory, is the common subterfuge of indolence, ignorance, and the sinister pretensions of empiricism. It is impossible, without some degree of indignation, to hear physicians asserting that an animal body, unlike other parts of nature, is governed by no regular and immutable laws; that medicine is merely a conjectural art; that it can boast of no definite or established principles; and that professional ability depends on some rare, intuitive, incomprehensible felicities of mental character, not reducible to principles, nor capable of being taught to others. Such assertions are not only derogatory to the profession; but they are destitute of truth. And, perhaps, it would be impossible to express this in stronger terms than those adopted by the author of *Zoonomia*, who remarks, that “there are some modern practitioners who declaim against medical theory in general, not considering that to think is to theorize; and that no one can

"direct a method of cure to a person labouring under disease with-
 "out thinking, that is, without theorizing; and happy, therefore, is
 "the patient, whose physician possesses the best theory."

The authors of the work before us, devote the first part to the establishment of the principles which are to guide all their subsequent reasoning and practice. In doing this, they generally follow the fundamental doctrines of the Brunonian system. The light they throw on many parts of that system, and the zeal they display still further to improve as well as to commend it to public attention, reflect much credit on their ingenuity and diligence.

It would not consist with our limits on this occasion, to attempt a critique on the Brunonian theory. As its reputation, considered as a whole, is certainly on the decline, we are as anxious to do justice to its merits as to censure its faults. The praise of an original, profound, and luminous understanding, undoubtedly belongs to Dr. Brown. It gives us pleasure to find his name enrolled in the catalogue of those illustrious men, who have extended the landmarks of medical science, and committed the memory of their labours and discoveries to the gratitude of posterity. We remember, with regret, the adversity which clouded the best of his life, and repressed the noble elevation of his mind; and we draw a veil over the frailties that so greatly impaired the force and shortened the duration of his wonderful faculties.

But as the attempt, in the work before us, to revive the popularity of the Brunonian principles, may possibly fall into the hands of some not prepared to detect the mischief which lies concealed under a specious exterior; it may not be improper to make a few remarks, by way of caution. We pass over in silence, the objections to Dr. Brown's system, arising from his hypothesis of a fixed original stock of excitability, uniformly and equally distributed over the whole system, in diseases as well as in health; his doctrine of uniformity in the operation of stimulants; his inattention to the great range of the operations of the absorbent system; and his neglect of the sympathetic associations in the animal economy, which are of such importance in explaining many intricate phenomena, both of health and diseases. These mistakes and imperfections, however defective they may render his system, cannot be supposed to lead to much practical mischief.

Although our authors adopt the fundamental principles of Dr. Brown, they dissent from him in some particulars. The principal deviations are the following. 1. They contend that diseases of excessive excitement cannot exist; and that all those, which have been so called, are diseases of indirect debility. 2. They maintain that almost the whole of the diseases which were ranked, by Dr. Brown and his followers, among the diseases of direct debility, are diseases of indirect debility. 3. They also differ from

him in the mode of applying the exciting powers, for the cure of diseases of indirect debility. Presuming that our readers are well acquainted with Dr. Brown's Elements, we deem it unnecessary to specify his doctrine concerning these topics.

Passing over many points of smaller consequence, we cannot dispense with the duty of remarking on one leading principle in Dr. Brown, adopted and greatly extended by the authors of this work. In treating diseases of indirect debility, they state, that "As the body becomes less susceptible of impression, in the direct ratio of the excessive application of stimuli, it follows, that the force of stimulus to be applied, in the cure of diseases of this state, should be directly as the exhaustion of the excitability."—Taken in a general and unqualified sense, this proposition is liable to the most pernicious abuses; and when we observe the catalogue of diseases assigned to this class of indirect debility, our attention is still more forcibly arrested. Whatever mischief may have arisen from the application of Brunonian principles to practice, we are persuaded a large proportion of it has sprung from this source. If the animal system were a more simple, homogeneous mass of matter, endowed with excitability uniformly diffused over the whole, incapable of partial accumulation or exhaustion; and if the excitement resulting from the application of stimulants were consequently equal and uniform throughout the entire system; this principle would have a good foundation. But the reverse of all this is found to be true. The animal body is a compound, heterogeneous and dissimilar fabric, made up of very diversified organization, possessing excitability capable of great accumulation, and, consequently, of great excitement in one or more parts, while others are left in a state of proportionate debility from exhaustion. Violent stimulants applied to one part will excite morbid action in that part; and, at the same time, extend this morbid influence to other parts with which it is connected by sympathetic association of motions. The source of excitability (supposing the brain, with its appendages, to constitute this source), irritated by such excessive stimulation, sends forth a larger quantity of this vital power, which, being chiefly transmitted to the morbid parts of the system, renders the action of such parts more violent, until often, through the medium of inflammation and its consequences, the affected organ is rendered unfit for the purposes of life. Here, then, is a complicated disease, whose nature and treatment cannot be decided upon first impressions. If powerful stimuli be applied, to relieve the indirect debility which pervades so large a portion of the system, the primary and principal seats of disease, overwhelmed by such pressure of excitement, will suffer a fatal congestion, effusion, engorgement, extravasation, or gangrene. To restore the balance of the systems, and to guard those parts from injury, which are

exposed to the principal stress of the disease, is the object to be held in view; but this object, we apprehend, will not often be accomplished by powerful stimulants. Life is maintained, and the functions of the system regulated, by the successive supplies of excitability being equal to its waste or expenditure in the perpetual operations of the vascular organization. The remedy provided by nature, for the increased excitement of any part of the system, is the consequent expenditure of excitability. Excessive action, if it can be safely supported for the requisite time, and in requisite degree, will gradually wear away the superabundant irritability, and in this manner bring down the system, and leave it at the point of health. But how many fatal occurrences may take place in the course of this natural process! Provision is made by nature, for sending a very large quantity of blood to the brain, in order, probably, to secrete the matter of life, which is incessantly consumed by every moving point in the body. A small variation of this quantity, by excess or diminution, is, on good ground, supposed to be extremely hazardous. This delicate, vital secretion may be readily overwhelmed by congestion, engorgement, or effusion; modes in which it is probable arterial violence often proves fatal in fevers, and which are indicated by the different degrees of comatose affection, so frequently observed in unfortunate cases. But if the fountain of life should escape, the same event may easily be produced by the congestion, inflammation, and effusion, incidental to other viscera from this vascular tumult. In a word, it appears probable that fevers scarcely ever prove fatal, except where local destruction is in some such manner produced; which, with prudent practitioners, will always be an objection to powerful stimulants in cases of too great arterial force.

Drunkenness affords a familiar example of what Dr. Brown calls indirect debility, or excessive expenditure of excitability. It may serve as an epitome of a malignant fever. Like such a fever, it is owing to a violent stimulus, which produces more energetic motions of the arterial, venous, glandular, and absorbent systems, great prostration of strength in the voluntary motions, staggering, stammering, delirium, apoplectic stupor, and finally great expenditure of excitability, from which the inebriate only gradually recovers, after many hours. Nothing but a more deeply-rooted and virulent cause, and a more protracted disease, are wanting to render the resemblance complete. And yet, we believe, it has been seldom proposed to relieve the indirect debility of drunkenness, by strong stimulants, while any part of the system continues to be considerably excited. Quiet, repose, dilution, cool air, and, in some cases, cold water applied to the surface of the body, have been supposed to be the best remedies. But in such a case,

the necessity of exercising caution in the use of stimulants is not so evident as in regard to the fevers called malignant, and many other diseases of indirect debility. The operation of ardent liquors is generally transient, and rarely endangers the organic derangement of the vascular system, upon which the fatality of fevers often depends. It is indeed, true, that when a fit of intoxication is over, to relieve the languor which always more or less ensues, recurrence is often had to spirituous drink, and that for a time it answers the purpose. But here, before the repetition of the draught, the violent commotion of the system has subsided, and a universal torpor taken place. An illustration of our opinion, similar to this afforded by drunkenness, may be derived from violent exercise or labour, from excessive repletion, from the tumult of anger, &c. In all these cases, we conceive, inordinate excitement should as much as possible be restrained, and every movement brought down to a calm and natural state. The evident injurious effects of ardent liquors, when taken to support the strength of such as are addicted to hard labour; and the experience of Europeans in hot climates, that far greater exertions can be made by persons who chiefly eat vegetables and drink water, than by those who live more freely, appear to confirm our opinion.

The vital power or excitability in animals is an *unique* in nature. On this account, it has been difficult to call in the aid of analogy to explain the intricacies which envelope the subject. The term excitability, given, by Dr. Brown, to this principle of life, has led, we believe, to a number of mistaken views. It appears inherently to possess a great degree of activity, velocity, and vigour. The name he has thought proper to bestow on it, and the impression he gives of life as a forced state, have made this principle be considered as naturally more passive and dormant, when unexcited by stimuli, than a correct examination of facts will warrant. It is indeed admitted, that the presence and application of certain substances, which may perhaps more properly be called *nutritive* than *stimulant*, are essentially necessary to the existence and activity of this principle of animation; just as oil is necessary to preserve the flame of the lamp, or oxygene to support the process of combustion. Animals are constantly immersed in the fluids of heat, of the atmosphere, or of water; and alimentary matters are frequently necessary to repair the waste which the actions of life are continually making. But if a few articles of nutriment or sustenance, such as these, be granted, which are of the first necessity, the principle of life will exert a preserving, resisting, and recovering power; and the functions of the system will proceed with a degree of force and regularity, greater or less, according to existing circumstances. These observations are made with the view of inferring, that when the body is attacked by a morbid stimulus, if the excitement can

be reduced within certain moderate bounds, the inherent powers of the vital principle will soon be sufficient to restore good health.

We now proceed to consider another doctrine maintained in this work, the establishment of which is laboured with great zeal, viz. "That diseases of excessive excitement cannot exist; and "that all those which have been so called, are diseases of indirect debility." * This proposition has so much the air of paradox, that many will suppose a serious refutation of it ought not to be attempted. It is wonderful, that such a fallacy as this should so far impose on the minds of our respectable authors, as to induce them to deny that phrenitis, pneumony, acute rheumatism, small-pox, measles, &c. are diseases of excessive excitement, or that they should be treated otherwise than by stimulants. They suppose this to be proved by the powers known to induce those diseases, and by the remedies found most successful in their cure. The three former of the diseases just mentioned, they believe to be commonly occasioned by the alternation of cold and heat; and, as a high degree of heat, succeeding the exposure of the body to a low degree of cold, will rapidly expend the excitability, they contend that great exhaustion must necessarily take place: but they forget that this morbid exhaustion is produced through the medium of morbid or excessive excitement, and that this excessive excitement, in its turn, will occasion, for some time at least, the production of an excessive quantity of excitability, thereby mutually increasing each other. The "most successful mode of treatment of these diseases, viz. by warmth, small quantities of opium, wine, &c. and by the application of fomentations, rubefaciants, and blisters, to the local affection," they suppose, confirms their doctrine. For the credit of the profession, and the safety of mankind, we hope this treatment is seldom employed. "The languor, inability to move, want of appetite, nausea, costiveness, &c. which "occur in these diseases," are supposed by our authors, "to be "evidently incompatible with such a state as that of excessive excitement." It is admitted, that, in this case, the muscles of voluntary motion, the stomach and other portions of the alimentary canal, and perhaps other parts of the system, are exhausted of their excitability, and extremely weak; but, at the same time, the arterial, venous, glandular, and absorbent systems, are subjected to the most excessive and alarming action. This distinction, unattended to by Dr. Brown, or the authors of the work before us, we conceive to be most important—that the voluntary muscles and the stomach, &c. may be so weak that the patient cannot move a limb, or retain a particle of food; while, at the same time, the arterial system may be overstrained, convulsed, and lacerated by excessive action. The violence of the disease, in these cases, concentrates the excitement and excitability in the vas-

cular
nature
M
prin
and i
T
the g
imag
fresh
consi
veyin
the p
conve
other
mane
cate r
Fla
cont
small
indire
Gro
excite
exac
betwe
fuel;
tort su
the po
timate
dinary
will e
tingui
But
dered,
by the
derang
ple an
the co
able in
ferent
ground
One d
citeme
indirec
miss t

cular system, leaving the other parts of the body deprived of their natural share, and consequently in a state of indirect debility.

Mr. Christie has devised a scheme to illustrate the fundamental principles of the Brunonian system, which appears to be apposite and ingenious.*

The analogy of a fire-place is employed for this purpose. Let the *grate* represent the *human frame*—the *fuel*, the *excitability*—an imaginary *tube*, opening into the grate from behind, and supplying fresh fuel, exhibits the *source* of the excitability—an *air-machine*, consisting of many *tubes*, leading into the grate in front, and conveying air, denotes *stimuli* of different kinds and quantities—and the production of *flame* represents *life*. If some of the air-tubes convey oxygene gas, they will denote the diffusible stimuli—if others convey atmospheric air, these refer to the ordinary and permanent stimuli—and if others again carry impure air, these indicate miasma, contagion, poisons, &c.

Flame, or *fire*, denotes *life*—*life* is produced by *stimuli*, and continually wastes *excitability*. A large quantity of fuel, and a small fire, point out *direct debility*—a large fire and scanty fuel, *indirect debility*—and a just proportion of both, *good health*.

Great violence of the fire will represent diseases of excessive excitement; but in order to preserve the resemblance with any exactness, it becomes necessary to imagine some connecting power between a violent fire and an increased, preternatural supply of fuel; as certain degrees of diseases of high excitement always exert supplies of excitability preternaturally great, and thus acquire the power of maintaining and increasing their own violence. Ultimately, however, in the progress of such diseases, this extraordinary supply of excitability will fail; this violence of stimulation will exhaust the whole of it, and the flame of life be thereby extinguished.

But Dr. Brown and his followers do not seem to have considered, that the grate might be so constituted, as to be destroyed by the violence of the fire; as the human body may suffer organic derangement and destruction by excess of excitement. The simple and uniform material constituting the grate, contrasted with the complexity, intricacy, and delicacy of organization discoverable in the animal system, and the consequent liableness to different states of morbid action in different parts, is also another ground of the oversight and mistakes of the Brunonian theory. One department of an animal body may invite and concentrate excitement of the most furious kind, while others are torpid from indirect debility. The stimulants applied to rouse the torpid parts miss their aim, and expend all their force in overstraining and

* Beddoes's Preface to Brown's Elements, p. 129.

destroying the vascular organization. A more natural management seems to consist in closing some of the air-tubes, and thereby reducing the fire within safer bounds; or, in other words, to withdraw a proper quantity of stimulus, and leave the system to regain its proper equipoise.

The general principles entertained by our authors are also applied to local diseases. Considering local inflammation not as a disease of excessive excitement, but of indirect debility, they contend for the propriety of stimulant applications. Among other local inflammations, this doctrine is extended to burns and scalds. A few facts, out of those constantly passing before our eyes, will be sufficient to overthrow all the reasoning on this point. The recollection of every practitioner in the United States will, no doubt, supply additional instances, though perhaps none more remarkable than the following. The lady of the Commandant on Governor's Island, the last winter, inadvertently burned one of her fingers, the whole length, against an iron. The surgeon of the garrison, who was present, immediately caused it to be placed in cold water, which was frequently supplied, for a few hours, with the entire cure of the burn. In the same season, some of the people of the garrison were employed in killing hogs; and a large quantity of hot water was prepared, as usual, to scald them. By accident, a pailful of this scalding water was overthrown on the foot of a soldier. Cold water was immediately dashed upon it, and frequently renewed for half an hour, when he was able to proceed with his business. Many years ago [one of the Editors of the Repository received the facts from the persons concerned] two brothers, apprentices to a hatter, were employed in taking new hats from a boiler, and rinsing them out in a very large tub of cold water. Some dispute arising, one of them lifted the other by his arms, and seated him directly in the boiler; but being instantly struck with terror at what he had done, without loosening his hold, he again lifted him from the boiler, and seated him in the tub of cold water. The youth who had been thus hurried through these extremes of temperature, had on a pair of wide linen trowsers, and received no other injury than a narrow blister, which was formed directly under the waistband, and encircled his body.

In order to recapitulate and condense the reasoning employed on this intricate subject, which we cannot but think very important in its influence on the practice of medicine; we repeat our dissent from Dr. Brown's unqualified doctrine of *treating diseases of indirect debility by stimulants*; and the still more extraordinary doctrine of the work now under review, *that diseases of excessive excitement cannot exist, and that such as have been so considered are to be treated by stimulants*,—for the following reasons.

1. Because, under the mask of great indirect debility in the muscles of voluntary motion, in the alimentary canal, and perhaps in some other parts of the body, violent and destructive excitement may exist in the vascular, and especially in the arterial system. And, to relieve the debility, at the expence of aggravating the excitement, if it were even practicable, would be to sacrifice the vascular system for the sake of invigorating the locomotive muscles and the alimentary canal.

2. Because Dr. Brown and our authors do not appear to advert to the co-existent circumstances of copious production and waste of excitability, when any extensive or important department of the system is violently excited by disease. And this two-fold operation probably proceeds furthest when inflammation, or some other local derangement, is interposed; but the waste eventually much exceeds the production, which explains the universal debility immediately succeeding the solution of all violent acute diseases.

3. Because, when any extensive and important part of the system, as, for example, the vascular, is excessively excited, whatever stimulant remedies may be applied to relieve the deceitful debility of other parts, so far from relieving it, they will be in hazard of directly adding the sum of their stimulus to the stimulus of the morbid cause, and thereby producing an aggregate of the most violent and fatal excitement.

4. Because, even if we suppose the excitement of the most excited parts of a diseased body to be so moderate, that some stimulant remedies may be admissible and necessary, still the *powerful stimuli*, recommended by Dr. Brown, must be hurtful; for remedies of such force, though they increase excitement, and produce excitability at the same time, will always occasion much useless motion, and thereby the waste of excitability will always eventually preponderate the production of it, and the system become consequently more exhausted.

These mistakes of Dr. Brown and his followers, if they be really so great as appears to us, ought to be corrected. We think them as repugnant to experience as unfounded in principle. It is well known, that many physicians have long conformed their practice to these doctrines, and, to the present moment, hold the truth of them to be incontrovertible. With what pernicious effects principles like these must approach the bed of sickness, we shall not venture to inquire. The history of the abuses of stimulants would form, perhaps, as dark a page in the annals of medicine, as in those of general society.

The more maturely this subject is considered, the more importance will be attached to it; and with the more propriety will be

adopted and applied to medicine, the sage advice so fatally forgotten by the rash, inexperienced, and ill-fated Phæton,

Parce, puer, stimulis, & fortiter utere loris.

The unbecoming and undeserved censure, which the authors of this work have cast on the *Zoonomia*, must not be passed over in silence. Individual complaints, that a system is deficient in method, confused, and unintelligible, amount to but little. Before we admit them to be well-founded, we must inquire for the specific form of the charge. Such is the deplorable ignorance of some of the Tartarian tribes under the dominion of Russia, as we are told, that they cannot enumerate beyond three. That a savage of these hordes should find the tables and reasonings of Euler and of Newton confused and unintelligible, could certainly not be admitted as evidence of the validity of his objections. Such, in fact, is the structure of some minds, that they can form distinct conceptions only of the most naked and simple propositions, for the verification of which the senses may be directly appealed to. The abstract puzzles and confounds them; and every attempt to explain and illustrate, however scientific and ingenious, but adds to their embarrassment, and augments their perplexity. Mole-eyed, they descry a little way before them through the dim glimmering of twilight, but are dazzled and blinded by the full splendour of the glorious day. Without pretending that these remarks are applicable to the authors of the work under consideration, we cannot hesitate to declare our entire dissent from the opinion pronounced by them on the relative merits of the *Elements of Medicine* and the *Zoonomia*. To elevate the first above the last, is to prefer the rough, irregular, but masterly sketch, struck out by some sudden effort of untutored genius, to a design conceived, by some master, with the boldness of Angelo, and executed with the felicity of Raphael—in which, if all the parts bear not exact proportion to each other, if some minutiae are neglected, if a light more glowing falls on some particular figure; yet each is in itself entire and perfect, and the whole forms an assemblage of beauty, novelty, and grandeur, on which the long succession of ages pause with wonder and delight.

(To be continued.)

N. B. *When the review of this work was undertaken, we supposed it to have been originally published in this country, from the title and the manner in which the printer's advertisement is expressed; we since have found this was a mistake; but as the work is interesting, it has still been determined to adhere to the first design,*

ART. II. *Observations on the Doctrines of Phlogiston and the Decomposition of Water. Part the Second.* By Joseph Priestley, LL. D. F. R. S. &c. &c. Philadelphia. Dobson. 1797. 8vo. pp. 38.

NOTWITHSTANDING the answer of Citizen Adet, (see our Review, No. II. p. 225.) to the first publication of Dr. Priestley, (ibid. p. 221.) and the reply of Professor Maclean to the same work, (see our Review, No. III. p. 348.) this antagonist of the antiphlogistians still maintains his ground, and again bids defiance to his philosophical adversaries. He contends that he has not yet been fully and fairly answered, and calls upon his opponents for something more solid and conclusive than they have hitherto advanced; observing, at the same time, that he is not the only person who, at this time, adheres to the doctrine of phlogiston: but that Messrs. Crell, Gmelin, Westrumb, and Meyer in Germany, and his friends of the Lunar Society at Birmingham, still maintain it.

The solution of iron in the vitriolic and marine acids, appears to Dr. Priestley, after all that has been said, to be incapable of explanation upon the antiphlogistic plan; and indeed it must be owned, that some of his objections to their interpretation appear almost, if not quite, insuperable. If they are really capable of being answered, the gentlemen who have undertaken to answer, have to answer over again.

On the constitution of *finery cinder*, the Philosopher of Northumberland considers the explanations of his opponents very incomplete, and far from being satisfactory. And really, when this *peculiar* substance is affirmed to be a mere oxyd of iron, while experiments lead to the conviction of its being something very different, we do not wonder that Dr. Priestley is dissatisfied with the account they have given of it.

Respecting the *calces of mercury*, he yet declares his belief, that inflammable air may be imbibed by any calx of mercury; that the metal is revived by it, and cannot be revived without it; and therefore some element of which inflammable air consists, which no doubt is phlogiston, is a necessary component part of that metal, and therefore, he says, of all other metals also.

Concerning the *composition and decomposition of water*, the same difference in point of fact seems to exist between the parties, as when we reviewed Dr. Priestley's First Part. He affirms, in opposition to the antiphlogistians, that with very pure dephlogisticated air, and a proportion exactly defined of the purest possible inflammable air, he procures drops of a stronger nitrous acid, than can be obtained by means of air less pure. And he denies, that the azotic, or phlogisticated part of the atmosphere, even should

any of this be present, has any thing to do in producing that acid. In this case of contradictory experiments, we suppose that both ourselves and Dr. Priestley must wait until we hear from Mr. Berthollet and the other French chemists, to whom his first treatise was addressed.

We forbear to make any observations on the apparent harshness and acrimony with which this controversy has been conducted between our author and Professor Maclean. Confident of the zeal for science, and the spirit of benevolence which both possess, we have no doubt the exceptionable parts of Mr. Maclean's pamphlet proceeded not from *design to offend*, but partly from *inadvertency* and partly from *warmth of argument*, and that if Dr. Priestley knew the motives, he would not hesitate a moment to excuse him.

Upon taking a view of the whole controversy as well as we are able, it appears to us, there are some phenomena which the antiphlogistic theory cannot explain; and that, therefore, Dr. Priestley has some strong holds, from which it will be difficult, if not impossible, to drive him. On the other hand, the advocates for phlogiston are far from being explicit and definite enough in the meaning of their term, and on that account, seem not perfectly to understand each other, nor to be clearly understood by their opponents.

ART. III. *The Young Chemist's Pocket Companion; connected with a Portable Laboratory, &c. &c.* By James Woodhouse, M. D. Professor of Chemistry in the University of Pennsylvania. Philadelphia. Oswald. pp. 56. 8vo. 1797.

THE performance before us affords a new proof of the prevalence of a taste for chemical researches in the United States. And it is one of the circumstances of recommendation to the *Young Chemist's Pocket Companion*, that it is intended to advance the knowledge of that science, by facilitating the means of making experiments, and of interpreting and understanding them. So laudable are all attempts of this kind, that we cannot forbear thinking the author has done service to his favourite branch of philosophy, by the present publication, which may induce many persons to make themselves acquainted with the chemical action of bodies, and thus become able experimenters. Elementary and practical essays of this kind, are highly useful for initiating beginners, and we are pleased to find Professor Woodhouse condescend to collect and arrange a series of experiments, calculated to allure the mind along from object to object, and beguile it, as it were, into an acquaintance with the principles of some of the most interesting phenomena of Nature.

The author has prefixed to this work, a catalogue of the substances and apparatus for making experiments, contained in the *portable chest*, connected with it. And the number and variety of these are such as to permit a great number of experiments to be made. A collection of so many chemical preparations, in so compact and handy an arrangement, may be exceedingly useful to almost every person who is fond of these kinds of researches.

The number of detailed experiments which Professor Woodhouse has given, is one hundred; in which he explains the properties of airs or gases, of alkalies, of acids, of earths, and of metals. The explanations are concise and generally correct. At the end of the experiments, is an advertisement of the Professor's lectures, given annually in the University of Pennsylvania.

As the work is intended for those who wish to become practically acquainted with the science of chemistry, we recommend it, and the *Portable Laboratory*, to the students and cultivators of experimental physics; not doubting that the younger class of inquirers will be considerably aided by it, especially if they peruse it, in connection with such systematical works as those of LAVOISIER, FOURCROY, and CHAPTAL.

ART. IV. *An Inaugural Dissertation on Gangrene and Mortification.*
By Francis K. Huger, &c. Philadelphia. *Ustick*. 1797. pp. 31.

THE first part of this dissertation is employed in exhibiting a detail of the opinions of the ancients, on the subject of gangrene and mortification. After these, we find a view of the principles and practice of Boerhaave, and his Commentator, Van Swieten. The want of precision, in all those writers, in regard to the doctrines of the vital power, and its fluctuating states of accumulation and exhaustion, and consequently to those of the varied effects of stimulants, renders many of their remarks indistinct and unintelligible.

After justly expressing his dissatisfaction with such opinions, our author proceeds to exhibit and to adopt the more correct principles of the late Mr. John Hunter.

He defines gangrene and mortification to be the "total extinction of sensation and action in a part," or, in other words, "the death of such part." And he supposes it to be produced by a disparity arising between the power and action of the affected part. To comprehend perfectly the opinion of Mr. Hunter and our author, on this subject, it will be necessary to recollect, that the "power" of Mr. Hunter corresponds with the "excitability" of Dr. Brown, and the "sensorial power" of Dr. Darwin; and that his "action" agrees

with the "excitement," the "exertion," or the "fibrous contraction" of the last mentioned writers.

Action is divided, by our author, into healthy, disordered, and diseased. *Disordered* action, he states to be an action of restoration, tending to remove or counteract whatever impedes healthy action; but diseased action always tends to destroy the animal machine. The propriety of this distinction between disordered and diseased action, we cannot perceive. Difference in the degrees of force of the noxious cause, and in the state of concomitant circumstances, we are persuaded, produces all the variety observable in morbid action.

According to our author, gangrene and mortification are of two kinds; one without inflammation, and the other preceded by it. The former generally takes place in persons debilitated by previous diseases, by improper modes of living, or in persons advanced in age. The principal object, in this case, should be to impart strength and vigour to the system. Nutritious diet, the Peruvian bark, wine, &c. are here recommended. We are surprized to find the virtues of opium, so highly praised in such cases by Mr. Pott and other eminent surgeons, intirely passed without notice by the author. But the caution given on the authority of Mr. Hunter, against too large doses of the stimulants employed, is, in our opinion, of great importance.

Of the second kind of gangrene and mortification, viz. that preceded by inflammation, there are, according to our author, two species. The first is produced by the action of inflammation, exceeding the powers of the part to support; and the second is the consequence of a specific inflammation. In the former, the proper treatment consists in preserving the balance between the powers and the action. In such cases, all irritating applications are to be avoided, and every exertion made to moderate the action of the part. This may be most speedily accomplished by blood-letting, purging, and other depleting remedies; and by preserving the system in this quiescent state by low diet. When this disease invades the extremities, would not compression of the principal artery of the affected limb be a powerful means of arresting the spread of the gangrene, till a sufficient reduction of the action of the whole system could be obtained? With the depleting and other debilitating remedies, our author mentions the external use of preparations of lead and opium; and seems to think they operate in reducing action on similar ground. But as the lead and opium are chiefly useful, when the application of them *succeeds* the depleting remedies, we expected to find our author referring their mode of operation to different principles. And, indeed, according to the quotation made from Mr. Hunter, he appears to have considered them as acting in a different manner.

The second species of the latter kind of this disease is the consequence of a specific inflammation; which the author directs to be treated by remedies suitable to counteract the peculiar diseased action; or by destroying and separating the contaminated part from the sound.

It seems to have been our author's principal aim, to give a compendious view of Mr. Hunter's doctrine on this subject. We regret that he has not accomplished his purpose with more perspicuity, order and precision.

ART. V. *Fourteen Agricultural Experiments, to ascertain the best Rotation of Crops: addressed to the Philadelphia Agricultural Society.* By George Logan, M. D. Philadelphia. Francis and Robert Bailey. 1797. 8vo. pp. 41.

THE fourteen experiments, mentioned in the title of this publication, the account of which occupies about one third of the pamphlet, appear to have been suggested by a prize question, proposed by the Philadelphia Agricultural Society; and were "intended to acquire a knowledge of a rotation of crops calculated to afford,

"First, the greatest number of profitable crops;

"Second, the greatest quantity of fodder to winter cattle;

"Third, the greatest length of time between manuring the same field—and,

"Fourth, such a variety of crops, as may not interfere with one another: but, by coming in regular succession, may afford the farmer and his family the most constant employ."

This knowledge Dr. Logan supposes himself to have acquired, by means of the fourteen experiments related at length, in the first part of his pamphlet; and he concludes the following rotation of crops to be that which is best calculated to answer the purposes just enumerated.

"1. Indian corn. 2. Potatoes and flax. 3. Wheat. 4. Winter barley, and after barley, buck-wheat, with clover and timothy. 5. Clover, two crops for hay. 6. Clover, one crop for hay and pasture. 7. Pasture. 8. Wheat. 9. Winter barley."

This plan the author recommends on the credit of his own successful experience; and considers as adapted, "within the ability of one family," to a farm of "one hundred and eighty acres of land." But for the particulars of his divisions of such a farm, of the proportions for each crop, and of the arguments by which he enforces his opinions, we must refer the reader to the work

itself. It will naturally suggest itself to his mind, that, as the rotation here proposed, is proposed with relation to the State of Pennsylvania more particularly, some variations will be indispensable in the application to other States. Still, the plan will deserve consideration; which we hope it may receive from the intelligent farmer in every part of the United States.

ART. VI. *Sketches on Rotations of Crops, and other Rural Matters. To which are annexed, Intimations on Manufactures; on the Fruits of Agriculture; and on new Sources of Trade, interfering with Products of the United States of America in foreign Markets.* Philadelphia. Charles Cist. 1796. 8vo. pp. 76.

THIS appears to be a new and enlarged edition of a pamphlet published some years since, but which we do not remember to have seen much circulated in America. Its contents are multifarious; and that our readers may be enabled to form a better idea of the work, we shall transcribe the several titles of the author.

1. Of the English old Rotation of Crops.
2. English new Rotation of Crops.
3. Comparison between the English old and new Rotations of Crops.
4. Of the American old Rotation of Crops.
5. American Fallow-Crop new Methods; with and without Maize.
6. Clover Seed.
7. Bean Drill.
8. Sowing Wheat on Clover.
9. Of Farm-yard Manure.
10. Cattle Stalls.
11. Barns.
12. Ice Houses.

Then follow Intimations on Manufactures, &c.

The Sketches are illustrated with various plans and tables; many curious and interesting notes are annexed, partly by the author, and partly by an agricultural friend in England; and the various subjects are apparently discussed with great practical skill and good-sense. The *Intimations* form a pleasing part of the publication, and bespeak a mind at once reflecting, patriotic and ingenious.

The author of this very respectable publication, we are informed, is John Beel Bordley, Esq.

ART. VII. *Treatise on the Yellow Fever; shewing its Origin, Cure and Prevention.* By Joseph Browne. New-York. Argus-Office. 1798. pp. 31. 8vo.

THERE is hardly any point on which medical writers are more agreed, than ascribing much of the epidemic influence of distempers to some chemical alteration in the atmosphere. The attention of physicians, which has of late, in America, been roused to explore the causes of yellow fever, plague and pestilence, has traced these varied forms of sickness, generally, to a vitiated condition of the air which the people breathe. This ancient opinion, on which there are some excellent observations in the works of Hippocrates, has, until very lately, been expressed in terms as general and indefinite as in the time of the Greek physician.

Within a few years, attempts have been made, and, we hope, not without encouragement and success, to analyze the atmosphere a little more minutely than had been done before, and to refer morbid phenomena to certain, precise, and known chemical changes in the air. A number of writers have offered the fruits of their speculations and labours to the public, some of whose performances we have noticed in the progress of our work. Mr. Browne is one of those who undertake to shew the exact condition of atmosphere necessary to produce yellow fever, and diseases of a similar nature and type. In the first section of his work, he endeavours to assign the cause of yellow fever, which he makes to consist in heat, and a deficiency of oxygenous air, or, as he calls it, *animal vital air*; or a surplusage of azotic, or, as he terms it, *vegetable vital air*.

If we understand Mr. Browne aright, he considers the atmosphere chiefly a *chemical compound* of these two airs (p. 7.); and "when any portion of atmospheric air comes in contact with any substance that has a greater affinity with either of its component parts, than these parts have for each other, a decomposition of this portion takes place, and a new union is formed." (P. 9.) The blood, passing through the lungs, attracts the animal vital air, or decomposes a portion of atmosphere proportionate to the oxygen absorbed. By this means, this acidifying principle taken in, overcomes and corrects the tendency of animal bodies towards an alkaliescent or putrid state.

He seems to consider oxygene as neutralizing blood in the same manner that carbonic acid neutralizes lime; and, of course, "the blood is less or more acrid, or fit for the purposes of life, as it is more or less neutralized with animal vital air, which is the basis of acidity." (P. 11.) Thus it happens, that if the blood is not sufficiently oxygenated, not only itself, but the secretions from it,

become exceedingly alkaline and acrid; particularly the bile, which, being secreted from venous blood, and, consequently, always of an alkaline quality, may occasionally become so acrid, by a deficiency of *animal vital air* to neutralize it and the heat of the season, as to produce all forms of febrile distempers, from fever and ague to plague itself.

In short, whenever there is not oxygene enough to saturate with its acid, that alkaline humour, the bile, yellow fever and its kindred diseases are brought on by the acrimony and putrescency of the liver. In consequence of which Mr. Browne denominates his animal vital air, the "grand corrector of putrescency."

Such is the author's idea of the cause of the disease upon which he treats. Without animadverting much upon the truth or probability of such an account, we should be glad to learn from Mr. Browne the proofs, that the two gases forming the principal part of the atmosphere, are ordinarily in a state of chemical union, as we believe it to be generally understood among men of science, that they are diffused through each other, but not chemically combined. We do not see, as clearly as our author, the alkalescency of the blood, nor, if we could see it, are we satisfied that alkalescency denotes putridity. The analogy of lime and carbonic acid, adopted to explain the operation of oxygene upon blood, and upon bile, and the effects of both, and particularly the latter, when deprived, more or less, of animal vital air, seems to us very unhappily chosen, and calculated directly to mislead the mind. Such remote analogies as these are little adapted to promote the interests of science.

After giving, from books, various accounts of diseases brought on by vapours exhaling from putrefying substances, which Mr. Browne thinks are illustrations of his doctrine of a redundancy of vegetable vital air, he proceeds, in the next place, to treat of the cure of yellow fever. As he thinks too little oxygene in the atmosphere will not acidify the blood enough, and then the bile will be increased in quantity, and become of a more alkaline and acrimonious quality, promoting putrefaction, exciting fever and ague, diarrhoea, dysentery, bilious fever, plague, &c. the primary object of the physician will be to empty the contents of the bowels, and throw into the atmosphere an additional quantity of oxygenous air. Without advising any particular prescription for accomplishing the former, he passes on to give an account of the decomposition of nitre, by means of the sulphuric acid, to accomplish the latter. He also proposes a decomposition of that neutral salt by heat alone, by putting it into a retort, and keeping them in a red heat.

As in the beginning, the fluids are particularly affected, he proposes to diminish their quantity by bleeding in considerable quan-

ties, and to repair their loss, by acescent liquors of a vegetable kind. And as an expedient that possibly might answer in cases where it might be wished that great quantities of vitiated blood could be drawn off, he hints at the practice of transfusion, whereby good sound blood from the veins (he ought to have said the arteries) of an healthy person or some animal, may be poured into the veins of a patient who loses a proportionate quantity of bad blood. He likewise strenuously recommends keeping up the perspiration, but goes into no practical details on the subject, nor tells how it best may be effected. To remove non-respirable air from the patient's chamber or bed, Mr. Browne proposes a condensing machine, of which he has given a figure, called an abstractor, which may be useful. And he judiciously recommends setting a quantity of quick-lime under the beds of persons labouring under yellow fevers, &c.

Lastly, Mr. Browne proceeds to the method of preventing the disorder of which he treats. For this purpose, he recommends cleanliness of persons, houses, streets, and docks. He advises the planting of trees to absorb vegetable vital air, and evolve animal vital air. He urges the importance of a plentiful supply of good fresh water, and affirms the whole river Brunx could be turned from West-Chester county, through the streets of New-York. This is a very important recommendation, and we hope our fellow-citizens will pay attention to it.

To render the constitution as little liable to pestilential attacks as possible, our author is of opinion, and herein we think him very correct, that flesh-meats, and particularly fish, should be avoided as food during the dog-days, as should likewise spirituous liquors; with the exception of beer, cyder, and wine. He cautions against going to houses crowded with people, as theatres, &c. during hot weather; and suggests to proprietors, the hint of decomposing nitre in them at such times.

With some force and much propriety, he inveighs against the wretched and abominable practice of interring the dead in vaults and church-yards, within the city of New-York, and directs that places of internment should be at some distance from a city; and corpses be covered with quick-lime. And he glances with a good deal of significancy at the preposterous method often employed of making vessels on board of which persons have been lately sick, perform quarantine. Mr. Browne's concluding paragraph, is employed on the agitated question of the *contagious* nature of yellow fever, which he does not consider in all cases contagious; yet, in certain cases, approximating so near to it, that it becomes difficult to draw the line. He seems to think there is something in it that looks like contagion, and that it is probable vegetable vital (azotic) air may be absorbed and stir up mischief in the constitution.

We shall just add, these pieces of chemical discussion please us. We hope Mr. Browne will pursue the subject. The field of inquiry is ample, and where there is so much room, we feel desirous he should continue his labours. The septic or azotic part of the atmosphere, and of all substances which contain the basis of vegetable vital air, is peculiarly important, and we hope its history will ere long be thoroughly investigated.

ART. VIII. *A Dissertation (Inaugural) on the Properties and Effects of the Datura Stramonium, or Common Thorn-Apple; and on its Use in Medicine.* By Samuel Cooper, &c. Philadelphia. Samuel H. Smith. 1797. 8vo. pp. 58.

THE subject of this dissertation is the plant, known, in different parts of the United States, by the various names of Jamestown-weed, French chesnut, stink-weed, and moon-weed, or moon-wort.

Mr. Cooper first delivers the history of the stramonium; next, relates a very considerable number of experiments, made with the direct intention of ascertaining its virtues; then, subjoins observations on the experiments made; and concludes with some facts and remarks on the use of stramonium in medicine. In every part of his essay, he evinces uncommon attention, patience, and fidelity, fertility of reflection, and ingenuity of conjecture. Indeed, we have not often met with an inaugural essay which so fully merits general reading. Notwithstanding, some defects are observable. But these seem rather ascribable to the limited period allowed to the author for this novel inquiry, than to any less excusable cause;—and these imperfections have been acknowledged by himself, with a degree of humility beyond what could fairly be required of him. In the hope that his essay will be widely circulated, and excite others to investigate such parts of the subject as remain to be explored, we shall be less minute in our examination than its importance would otherwise deserve.

Mr. Cooper's experiments were chiefly limited to the effects of the leaves of the stramonium. Some facts are adduced by him, principally on the authority of others, relative to the operation of the seeds, &c. From the concurrent testimony which he has reported, there remains no doubt of the highly stimulant powers of this plant; and that, judiciously applied, it may be advantageously added to the magazine of medical agents. The most obvious imperfection in the experiments, is in those which are designed to shew the operation of the expressed juice of the plant on the vascular and nervous systems, and on the various internal

parts of animals. It is difficult to determine, from the entire want of comparative trials (as, for instance, with water), how much of the effects produced are peculiarly attributable to the stramonium.

The XXXVIIth experiment deserves to be quoted at length, as it exhibits a very curious and remarkable fact.

EXPERIMENT XXXVII. (See p. 30.)

"I obtained a dog that laboured under periodical twitchings or convulsions, which occurred every three or four minutes. He appeared to be old; his frame was emaciated, his countenance dull, and, when he walked, his steps were slow and irregular. To this dog I gave a scruple of the extract of the leaves, which was repeated several times. The effects of the medicine were frequent black stools, a frequent flow of darkish urine, thirst, languor, and emaciation of the body. But this treatment did not diminish his convulsions. I now remitted the medicine for several weeks. During this period he became fatter, and more lively than I had hitherto seen him. Upon repeating the medicine in larger doses, similar effects as before, but of a more violent nature, were produced. His respiration became laborious and slow; and his death soon occurred. Upon opening his abdomen, a large worm lay naked to the view, except its extreme parts, which were concealed by the intestines. I was much astonished, and much delighted, at a discovery so unexpected; and at once concluded, that the worm had been the cause of convulsion in the dog. It was of a vermilion colour, above a yard in length, and nearly an inch in circumference. Professor Barton, so justly celebrated for his profound knowledge of the objects of nature, is of opinion, that this worm is a new species of *Ascaris*. He probably will be induced to describe and arrange it among the vermes.

"No perforation through the intestines was obvious; they were full of a yellowish fluid; but not any fluid was found in the cavity of the abdomen. May we not conjecture, that the worm perforated the intestines upon the first exhibition of the stramonium, which, according to Dr. Rush and Dr. Fowler, is offensive to worms. The external surface of the intestines and stomach were redder than natural. Some parts of the intestines, and much of the mesentery, appeared to be in a gangrenous state. A small quantity of water was found in the ventricles of the brain. These last effects, I think, may, in a great measure, be attributed to the violent operation of the medicine."

In the course of his observations on the experiments, which he details, Mr. Cooper seems inclined to ascribe to the thorn-apple the power of producing intermittent and remittent fevers, by its

peculiar exhalations. In this opinion we can not concur. Nor do we think that much can be derived in its favour, from the circumstances which he mentions, on the authority of Mr. Heckewelder and Colonel Sargent. The illness of the former, and of General Putnam, may be referred, with greater probability of truth, to the circumstances under which they had lived, previous to that event; and it will be a sufficient reply to the sentiment countenanced by the latter, that the stramonium delights to grow in soils and situations which are known to be unfavourable to health; that it appears to follow man in his migrations, and to his social establishments; and that, though these facts are found to be frequent, yet it does not always happen that the greatest abundance of vegetables of this species produce any bad effects on the health of persons living in their immediate vicinity.

In treating of the medicinal use of the thorn-apple, Mr. Cooper states several interesting cases, of the epileptic and maniacal kind, in which it was administered with some temporary benefit. But, as it was employed in conjunction with other and powerful remedies, it is impossible to form any certain conclusions from the facts adduced. The most important part of the practical information, here presented, is contained in a letter from Dr. John Archer, of Harford-County, Maryland. But, for this, we must refer the reader, as well as for a variety of other valuable information, to the dissertation itself; of which we can not take leave without again bestowing on it our sincerest praise.

ART. IX. *An Inaugural Essay on the Effects of Cold on the Human Body.* By John Edmonds Stock, &c. Philadelphia. Joseph Gales, 1797. 8vo. pp. 43.

AMONG those who are accustomed to examine, or called upon to decide on the merits of inaugural essays, there must be very few who are unapprized of the nature of their situation, whose duty obliges them to appear in this line of authorship. The pursuits common to students, and the general embarrassments which result from them, each must have, probably, experienced in his turn, and, therefore, be as able to estimate the one, as prepared to compassionate the other. These sentiments, which so naturally suggest themselves to the mind, when reflecting on this subject, should induce men not misled by the affectation of humility to lay aside those needless apologies which, as in many similar productions, form an unpleasing part of the essay before us. Neither is it so generally true, or, if true, so unavoidable, that the student confine himself in his inaugural discourse, to a track

beaten plain by the immemorial passing of physicians. Many fields of medical science, untrodden, or but partially explored, present themselves on all sides, and invite a survey, in which there is ample room to expatiate and observe. In every direction, subjects of experiment multiply themselves; for the investigation of whose history and nature, care, patience, and perseverance, are all the qualifications that seem necessary to be added to the usual attainments which precede graduation. With these, we have seen many useful and original discoveries effected.

Mr. Stock's essay is intended to comprize, first, the principal facts relative to the operation of cold upon the system, in a healthy state; and, secondly, the application of the facts, thus collected, to the regulation of its use as a remedy in a morbid state.

The author's primary intention is accomplished (in a manner more satisfactory for the style in which his narration is clothed, than for the order with which it is conducted), by a miscellaneous assemblage of facts and opinions from various writers; which leave no distinct impression on the mind, further than that the effects of cold are very considerable. A similar imperfection pervades the second part of his essay; which rather tends to direct us in the application of cold, as a remedy, by scattered examples, and heterogeneous opinions of others, than by any simple principle, rationally deduced, and practically applied. Mr. Stock, however, makes great use of the reasonings and authority of his celebrated preceptor, Dr. Rush. Hence, and from his having collected some facts not generally known, and from the number of well-known facts to be met with in the small compass of this essay, it may deserve to be considered as a publication of convenient reference; and as such, merit to be recommended to practitioners not under favourable circumstances for the consultation of numerous authorities.

ART. X. *An Experimental Inquiry into the Properties of Carbonic Acid Gas, or Fixed Air; its Mode of Operation, Use in Diseases, and the most effectual Method of relieving Animals affected by it. Being an Inaugural Thesis. By Joseph Johnson, &c. Philadelphia. Utick. 1797. pp. 50.*

THE discoveries made within a few years concerning the constitution and properties of the æriform fluids, may justly be called the pride of modern philosophy. The diligence, zeal and penetration, which have been exercised on this subject, will reflect lasting credit on many of the inquirers; and the benefits that have already, and are likely still further to accrue to medicine, from these improvements, must give pleasure to every lover of mankind.

The author of this dissertation distributes his observations into three sections. In the first, he treats of *the manner in which Carbonic Acid operates on the animal body*. In the second, he delivers *the method of relieving animals apparently destroyed by Carbonic Acid Gas*. And, in the third, he speaks of *the use of Carbonic Acid in the cure of diseases*.

1. Presuming that the reader is well acquainted with the chemical constitution of carbonic acid gas, our author proceeds to treat of the manner in which it operates on the animal system. Many philosophers had supposed, that this gas destroys life merely by depriving the system of oxygene; but this opinion is opposed by facts leading clearly to a contrary conclusion. The supposition of its acting as a sedative is also overthrown by arguments which firmly establish the stimulant operation.

In order to ascertain the mode in which this substance acts on the living body, the author made a variety of experiments on himself and others. Different quantities of water, more or less strongly impregnated with it, were taken into the stomach; and in this way it operated in a very speedy and sensible manner. Received by injection into the rectum, it produced effects very observable, but less than those taking place in the stomach. When applied to the skin under bed-clothes, and afterwards, in a larger quantity and more concentrated state, by the experimenter entering a brewer's beer-tub, it operated so manifestly as to remove all doubt of its force with respect to the skin. Admitted to the eye, it soon occasioned severe smarting. In a puppy, apparently dead, a small quantity injected through an opening in the pericardium instantly renewed the contraction of the heart. But in no mode of application did the author perceive such sudden and powerful effects as when he inhaled it into his lungs.

When applied in the several modes that have been mentioned, and in moderate quantity, the carbonic acid always produced a greater or less degree of the following effects. It increased the frequency, fulness, and strength of the pulse; it raised the heat of the body; it produced exhilaration of mind; it excited throbbing of the temples; and, when the dose was more considerable, there took place great distension of the head and neck, redness and turgescence of the face, heat, redness and protrusion of the eyes, dimness of sight, and tendency to vertigo. If applied in still greater quantities, the effects become highly deleterious: the breathing is rendered difficult, laborious, and stertorous—the pupils are much dilated—languor and coma come on—convulsive twitchings succeed—the pulsations are weak, slow, and irregular—the motion of the heart begins to fail, and soon entirely ceases.

The similarity of these effects to those arising from excessive quantities of opium, datura stramonium, ardent spirits, and other violent stimulants, will easily be perceived.

The author supposes carbonic acid to prove fatal to animal life, by inducing apoplexy. This he infers from the universal excitement that takes place; from the obvious determination to the brain; from his own sensations of plethora about the head, the turgescence of the neck and face, the distended or protruded state of the eyes, the dilated pupils, the coma, and the laborious and stertorous respiration. He also infers it from the turgid state of the blood-vessels of the brain, and the effusions of bloody serum into the ventricles, in the dissection of animals destroyed by this poison. And, lastly, from the paralytic effects which have been observed to arise from exposure to less degrees of it.

2. In the second section, the author goes on to direct the method of relieving animals apparently destroyed by carbonic acid gas. Believing that death is produced in this case, "by the violent impression of a powerful stimulant on the body, occasioning an affection of the arterial system, similar to that in the most inflammatory state of fever, and a peculiar determination of the arterial action to the brain;" the author prescribes his remedies on this ground. He orders the jugular vein, or temporal artery, to be opened, a proper quantity of blood to be drawn, and ice, snow, or very cold water, to be applied to the head and to the whole body.

To establish the propriety of this practice, on the ground of experience, he adduces a striking case, thus treated with success in Philadelphia—the effects of the same treatment repeatedly tried on cats subjected to this poison—the experience of the Russians, and of the inhabitants of Siberia, in favour of the same plan—and, finally, the analogy of the successful management of the drunken apoplexy, by plentifully dashing cold water over the head and the whole body.

If arguments were wanting for that purpose, we might, from the success attending this mode of treatment, deduce a powerful objection to Dr. Brown's principle of applying strong stimulants in cases of great indirect debility.

3. The third section is employed in exhibiting the uses of carbonic acid in the cure of diseases. And here he adduces testimony of its efficacy in typhus, in confluent small-pox, in cynanche maligna, in dyspepsia, in the nausea and vomiting of fevers, in scurvy, scrophula, and dropsy. The use of this remedy has also been greatly extolled in phthisis pulmonalis; but so much contradictory testimony is to be found on this subject, that further experience is certainly necessary to adjust a number of doubtful points. In calculous complaints, the efficacy of this remedy deserves to be rated very high; as the evidence in favour of it comes from various sources, and is altogether unquestionable. From its specific operation on the brain, it is reasonable to suppose it would be

very useful in melancholia, and depressions of mind. In worms, there seems to be ground to expect benefit from it, as malted and other fermented liquors are said to destroy them. In obstinate ulcers, scrophulous or scorbutic, in cancer, and in gangrene and mortification, it has obtained a very high character.

We feel pleasure in observing that much useful matter is to be found in this dissertation. The promises of the preface, with respect to novelty and improvement, we apprehend, will scarcely be realized in the perusal. Small faults are, however, always venial, when they are confronted by substantial merit.

ART. XI. *Proceedings of the College of Physicians of Philadelphia, relative to the Prevention of the Introduction and spreading of Contagious Diseases.* Philadelphia. Dobson. 1798. 8vo. pp. 37.

WE insert this title principally with the design of informing such of our readers as may be desirous of possessing the transactions of this respectable body of physicians relative to this interesting subject, how their wishes may be gratified. The little pamphlet before us, comprizes all the proceedings of the College, in respect to the importation of diseases, from August 1793, to August 1797, in a very convenient form; and composes an essential part of the medical discussions which have arisen since the first-named period.

ART. XII. *An Inquiry into the Causes of Sterility in both Sexes; with the Method of Cure.* By James Walker, M. P. M. S. &c. Philadelphia. Oswald. 1797. 8vo. pp. 22.

THIS is a very imperfect and unsatisfactory performance; and, though short, unnecessarily expanded. The mistake, relative to Noah, (p. 17.) reminds us of a question seriously proposed, for forensic disputation, by a collegian, whose incredulity surpassed his memory—"Whether Noah was ever in the whale's belly?"

Mean
Do.
Do.
Great
Do.
Four
Three
Warm
Fol

METEOROLOGICAL OBSERVATIONS for *January, 1798,*
made by GARDINER BAKER, in the Cupola of the Exchange, in
the City of New-York.

Days of the Mon.	Thermom. observed at		Prevail. winds.		Clear.	Cloudy	Barometer observed at	
	Sun-rise.	2 P. M.	7 M.	2 E.			Sun-rise.	2 P. M.
1	36	37	E	E	2		29 79	29 59
2	31	37	W	W	1	1	29 47	29 47
3	32	34	N W	W	2		29 62	29 62
4	25	31	N E	N E		2	29 70	29 63
5	32	31	N	N		2	29 22	29 34
6	27	33	N	N	1	1	29 63	29 70
7	21	30	N	N	2		30 4	30 4
8	20	29	N E	E	1	1	30 4	29 90
9	34	39	S W	W	2		29 40	29 31
10	28	36	W	W	2		29 37	29 38
11	15	23	N	N W	2		29 50	29 45
12	20	33	W	S W	2		29 68	29 65
13	28	41	S W	S W	1	1	29 79	29 79
14	30	44	S E	S	1	1	29 87	29 87
15	37	46	S	S		2	29 77	29 70
16	45	51	S W	S W	2		29 40	29 49
17	36	44	W	W	2		29 69	29 77
18	30	34	N W	N W	2		30	30 3
19	15	25	N W	S W	2		30 3	29 81
20	24	32	S W	W	1	1	29 43	29 36
21	33	42	S W	W	2		29 50	29 60
22	34	41	S E	S	1	1	29 57	29 20
23	23	17	N E	N W	1	1	29 18	29 40
24	13	22	W	W	2		29 70	29 68
25	24	35	S W	S W	1	1	29 60	29 47
26	28	34	N E	N E		2	29 43	29 31
27	30	22	N E	N E		2	29 37	29 20
28	16	20	N W	W	2		29 38	29 58
29	17	23	N	E	1	1	29 95	29 95
30	24	31	W	W	1	1	30 2	30
31	30	31	N W	N W	2		29 93	29 93

Results of Meteorological Observations for January, 1798.

Mean temperature of the Thermometer at sun-rise, deg. 27 3 hund.

Do. do. of the do. at 2 P. M. 33 16

Do. do. of the do. for the whole month, 30 9

Greatest monthly range between the 16th and 24th, 38 0

Do. do. in 24 hours, between the 22d and 23d, 24 0

Four days it rained, and but a small quantity has fallen.

Three days it snowed, and about 22 inches have fallen.

Warmest day the 16th. Coldest day the 24th.

Fol. I. No. 4.

Q

METEOROLOGICAL OBSERVATIONS for February, 1798.

Days of the Mon.	Thermom. observed at		Prevail. winds.		Clear.	Cloudy	Barometer observed at	
	Sun-rise.	2 P. M.	7 M.	E.			Sun-rise.	2 P. M.
1	26	40	W	NW	1	1	29 60	29 57
2	30	44	SW	SW	1	1	29 50	29 30
3	32	39	NW	NW	2		29 44	29 57
4	32	42	SW	SW	1	1	29 34	29 11
5	28	28	NE	NW	1	1	29 29	29 50
6	22	40	SW	SW	1	1	29 53	29 30
7	29	21	NE	NE		2	29 32	29 46
8	0	12	N	N	2		29 93	30
9	3	15	N	N	2		30 4	30 6
10	10	26	NW	SW	2		30	29 91
11	28	34	S	S		2	29 53	29 40
12	30	33	N	NW	1	1	29 50	29 56
13	22	26	NE	NE		2	29 63	29 59
14	26	28	NE	N		2	29 45	29 40
15	25	28	NW	W	2		29 73	29 80
16	21	29	NW	NW	1	1	29 95	30 1
17	19	28	NE	NE	2		30 10	30 13
18	12	24	N	N	2		30 25	30 28
19	14	23	NE	NE	1	1	30 18	30 13
20	32	33	E	E		2	29 85	29 79
21	33	34	NE	NE		2	29 90	29 90
22	34	41	NW	W	1	1	29 52	29 52
23	32	40	W	S	1	1	29 75	29 50
24	33	38	NE	NE		2	29 50	29 52
25	33	36	NE	N		2	29 47	29 39
26	33	37	NW	NW		2	29 46	29 50
27	26	36	NW	NW	2		29 60	29 57
28	26	37	N	N	2		29 72	29 72

Results of Meteorological Observations for February, 1798.

Mean temperature of the Thermometer at sun-rise, deg. 24 6 hund.

Do. do. of the do. at 2 P. M. 31 8

Do. do. of the do. for the whole month, 27 57

Greatest monthly range between the 2d and 8th, 44 0

Do. do. in 24 hours, between the 8th and 9th, 29 0

Two days it rained, and a small quantity has fallen.

Five days it snowed, and about fifteen inches have fallen.

Warmest day the 2d. Coldest day the 8th.

METEOROLOGICAL OBSERVATIONS for March, 1798.

Days of the Mon.	Thermom. observed at		Prevail. winds.		Clear.	Cloudy	Barometer observed at	
	Sun-rise.	2 P. M.	6 M.	2 E.			Sun-rise.	2 P. M.
1	28	33	E	E		2	29 80	29 80
2	31	35	E	E		2	29 50	29 27
3	26	33	NW	W		2	29 70	29 72
4	27	40	SW	SW	2		29 70	29 66
5	33	38	W	NW	2		29 85	29 98
6	23	36	NE	NE	2		30 7	30 4
7	27	41	E	N	2		29 71	29 55
8	36	51	SW	SW		2	29 43	29 39
9	27	34	NE	NE		2	29 75	29 58
10	31	54	SW	NE	2		29 43	29 50
11	33	49	E	S		2	29 52	29 14
12	30	37	NW	NW	1	1	29 18	29 37
13	30	41	E	W		2	29 52	29 52
14	36	47	SW	S	1	1	29 53	29 30
15	43	38	S	NW		2	29 24	29 25
16	36	40	NW	NW	2		29 47	29 60
17	30	41	W	W		2	29 83	29 61
18	34	42				2	29 70	29 60
19	33	35	SE	E		2	29 67	29 48
20	39	49	NW	NW		2	29 29	29 29
21	36	38	E	NE		2	29 24	28 61
22	36	44	NW	NW		2	29	29 29
23	34	45	SW	S	2		29 66	29 63
24	40	47	E	E		2	29 43	29 15
25	39	44	SW	SE	1	1	28 93	28 95
26	36	49	NW	W	2		29 22	29 34
27	45	52	NW	NW	1	1	29 50	29 58
28	37	51	E	SE	2		29 89	29 96
29	41	47	E	E		2	29 97	29 97
30	39	62	NE	S	1	1	29 93	29 80
31	51	70	S	S	2		29 76	29 70

Results of Meteorological Observations for March, 1798.

Mean temperature of the Thermometer at sun-rise, deg. 34 4 hund.
 Do. do of the do. at 2 P. M. 43 10
 Do. do. of the do. for the whole month, 38 57
 Greatest monthly range between the 6th and 31st, 47 0
 Do. do. in 24 hours, on the 30th, 23 0
 Six days it rained, and a large quantity has fallen.
 Warmest day the 31st. Coldest day the 6th.

TABLE exhibiting the number of Patients received into the New-York Hospital, in each month, their Diseases, and the event of each Case.

January, 1798.

DISEASES.	Remain. from last Month.	Receiv. this Month.	Cured.	Died.	Dis. Dis. and Eloped.	Under Care.
MEDICAL.						
Pneumonia	7	10	4	1		12
Ascites	2	4	1			5
Mania	5	2				7
Dyspepsia	1	1				2
Gonorrhœa	3		3			
Apoplexy	1			1		
Anasarca	5	2		1		4
Catarrh	4	3	2			5
Diarrhœa	2		2			
Rheumatism	11	3	6			8
Syphilis	7	9	6			10
Enteritis	1		1			
Intermit. Fever	2		2			
Amenorrhœa	1		1			
SURGICAL.						
Fracture	2	5				7
Syphilis	10	6	3			13
Sore Legs	13	7	2			18
Frozen Limbs	6	11	4	1	5 by de- sire.	7
Wound	1	3				4
Ophthalmia	3					3
Contusion	1	1				2
Tumor	1					1
Lumbar Abscess	1			1		
Fistula in Ano	1					1
Burn	1					1

Result for January, 1798.

Remaining from last month,	90
Received this month,	67
	—157
Cured,	37
Died,	5
Discharged by desire,	5
Under care,	110
	—157

HOSPITAL.

561

February, 1798.

DISEASES. MEDICAL.	Remain. from last Month.	Receiv. this Month.	Cured	Died	Dis. Dif. and Eloped.	Under Care.
Rheumatism	8	1	7			2
Syphilis	10	4	6		1 elop.	7
Pneumonia	12	8	5	2		13
Mania	7	1	2			6
Tetanus		1		1		
Ascites	5	1	4			2
Gonorrhœa		1				1
Anasarca	4		2	1		1
Dyspepsia	2					1
Catarrh	5		3			2
Intermit. Fever		1				1
Diarrhœa		2				2
SURGICAL.						
Sore Legs	18	2	3			17
Syphilis	13	1	6			8
Frozen Limbs	7	3	3			7
White Swelling		3				3
Burn	1	1				3
Wound	4	1	3			2
Fracture	7		4			3
Ophthalmia	3		1		1	1
Contusion	2		2			
Tumor	1		1			
Fistula in Ano	1					1

Result for February, 1798.

Remaining from last month,	110
Received this month,	31
	—141
Cured,	52
Relieved,	1
Died,	4
Discharged by desire,	1
Eloped,	1
Under care,	82
	—141

March, 1798.

DISEASES.	Remain. from last Month.	Receiv. this Month.	Cured.	Died.	Dis. Dif. and Eloped.	Under Care.
MEDICAL.						
Rheumatism	2	1				3
Syphilis	7	1	2		2	4
Pneumonia	13	3	9			7
Mania	6	1				7
Ascites	2	1			1	2
Gonorrhœa	1	1	2			
Anasarca	1					1
Dyspepsia	1				1 reliev.	1
Catarrh	2	5	4			3
Intermit. Fever	1		1			
Diarrhœa	2		1	1		
Jaundice		1		1		
Debility		2	1			1
Palsy		1				1
SURGICAL.						
Sore Legs	17	4	3		1 dis.	17
Syphilis	8	3	4		1 elop.	6
Frozen Limbs	7	5	2		2	8
White Swelling	3					3
Burn	2		1			1
Wound	2	1			2	1
Fracture	3	3	4			2
Ophthalmia	1	1	1			1
Scrophula		1				1
Fistula in Ano	1					1
Empyema		1				1
Disordered Spine		1				1
Cancer		2				2

Result for March, 1798.

Remaining from last month,	82
Received this month,	39
	—121
Cured,	35
Died,	2
Incurable,	1
Removed to the Alms-House,	1
Discharged disorderly,	1
by desire,	2
Eloped,	4
Under care,	75
	—151

HOSPITAL:

563

SUMMARY

For January, February, and March, 1798.

There remained under care, of last Year's Patients, } January 1, 1798,	90
There have been received, in the three last months,	137
Total	227

Of these have been cured,	124
relieved,	1
have died,	11
Removed to the Alms-House,	1
have been incurable,	1
have been discharged disorderly,	1
have been discharged by desire,	8
have eloped,	5
There remain under care, April 1st,	75
Total	227



A RETURN of Patients admitted to the Care of the New-York City Dispensary, from the 1st of January, to the 1st of April, 1798.

JANUARY.

DISEASES.	No.	Cured.	Reliev.	Died.	Removed to the Hospital.	Result.
Syphilis	4	4				Received 38
Cough	2	2				—
Pneumonia	4	4				Cured 29
Worms	1	1				Relieved 2
Pulmonary Consumption	2				2 u. care	Hospital 2
Hætic Fever	1	1				Under care 5
Rheumatism	1	1				—38
Concussion	2	2				
Erysipelas	1		1			
Catarrh	5	5				
Small-pox	4	4				
Anasarca	1				1 u. care	
Spitting of Blood	1	1				
Sore Legs	3				{ 1 Hos. 2 u. c.	
Quinsey	1	1				
Abscess	1	1				
Jaundice	1				1 Hos.	
Ophthalmia	1	1				
Fluor Albus	1	1				
Atrophy	1		1			

FEBRUARY.

Wounds	2	2				
Abscess	1				1 Hos.	
Mumps	1	1				
Spitting of Blood	1	1				
Jaundice	2				2 Hos.	
Diarrhœa	2	2				
Indigestion	3	3				
Pulmonary Consumption	1			1		
Burns	1	1				
Fluor Albus	1	1				
Hætic Fever	1				1 u. care	
Catarrhal Fever	3	3				
Herpes	1	1				
Small-pox	1	1				
Rheumatism	2	2				
Anasarca	2	1			1 u. care	

DISPENSARY.

565

FEBRUARY *continued.*

DISEASES.	No.	Cured.	Reliev.	Died.	Removed to the Hospital.	Result.
Syphilis	1	1				Received 31
Contusion	1	1				—
Hooping Cough	3	3				Cured 25
Costiveness	1	1				Died 1
						Hospital 3
						Und. car. 2—31

MARCH.

Pulmonary Consumption	3		1		{ 1 Hos. 1 u. c.	Received 43
Syphilis	2	2				Cured 40
Rheumatism	3	3				Relieved 1
Erysipelas	3	3				Hospital 1
Catarrhal Fever	7	7				Under Care 1
Worms	1	1				—43
Small-pox	8	8				
Fluor Albus	2	2				
Abscess	2	2				
Head-ach	2	2				
Herpes	1	1				
Mumps	1	1				
Dysentery	1	1				
Indigestion	2	2				
Scald Head	1	1				
Pneumony	3	3				
Colic	1	1				

SUMMARY.

Admitted,	112
Cured,	94
Relieved,	3
Removed to the Hospital,	6
Died,	1
Under care,	8

Total 112

HUGH McLEAN.

DEPT. OF THE ARMY

427,3433

[illegible]

MEDICAL NEWS.

DOMESTIC.

UNDER this head, in our last Number, we laid before the public the sum of our information respecting the *madness* then prevalent, in many parts of the country, among Dogs. Since that time, this disease, though it has disappeared from many places where it was then common, has shewn itself in various other places, in very different and remote districts of New-England. Much mischief has been produced by it. But our intelligence is not sufficiently particular to afford much satisfaction to the inquisitive reader. Such steps, however, have been taken, as lead to an expectation that no material facts will eventually be withheld. In the mean time we shall briefly state such facts as have been communicated to us.

From Rhode-Island we learn that one instance of death, from the bite of a mad dog, has occurred there, with all the symptoms of hydrophobia; and that another person was likely to perish in the same way; and that a yoke of oxen had been destroyed in consequence of the bite of the same dog. A third person had also been bitten by this dog, but had not yet suffered any inconvenience from the accident.

In Berkshire County, Massachusetts, many domestic animals have been bitten by dogs supposed to be mad,—and which were killed in consequence,—but no injury has hitherto happened to the creatures bitten.

In Connecticut the rabies had first shewn itself, in the town of Berlin, (about twelve miles from Hartford) in the beginning of March; though it had vanished from Hartford, and the places originally visited by it. We have not heard whether any serious injuries have been sustained there.

In the neighbouring town of Southington, the following melancholy accident has just occurred from this source. The account is copied from the Connecticut Courant for April 23.

“*Hartford, April 23, 1797.* Died, at Southington, on Sunday, the 15th of inst. April, Levi Woodruff, aged 43.

“The progress and origin of his disorder will probably be thought worthy of being recorded.

“He felt himself considerably indisposed on Thursday, was affected with a pricking pain in his ear, head-ache, &c. On

" Friday he was abroad. At evening, feeling his indisposition in-
 " crease, a physician was called in.—From a slight degree of *hy-*
 " *drophobia*, the physician was alarmed with an apprehension of
 " canine madness; but having never heard, though a near neigh-
 " bour, of the patient's having been exposed to be bitten, he sug-
 " gested nothing, at that time, of his fears. On the morning of Sa-
 " turday he saw him again, and, observing an increase of the same
 " symptom, he suggested the idea to the family. It was then re-
 " collected by them, that, for a length of time, the patient had ac-
 " customed a small dog, belonging to a neighbouring family, to
 " lick an ulcerated sore *within* his ear—that four weeks previous
 " to that time the dog was put to lick the sore—that the operation
 " was more than commonly painful, and that a small breach was
 " made in the skin of the ear, by the tongue or teeth of the dog—
 " that the next day the dog was affected with symptoms of mad-
 " ness, and the day following was killed. No apprehensions of
 " mischief were entertained at the time, nor had it been thought
 " of after. The physician was now convinced that the disorder
 " must have proceeded from the virus infused by the dog; but
 " this conviction was obtained at too late a period to admit of an
 " effectual application of remedies. Through the whole of Sa-
 " turday he was constantly craving water; but was thrown into
 " the most violent agitations and horror upon the sight of it.
 " Through the night he was very restless. In the morning his dis-
 " order increased to an height astonishing to all the spectators.
 " He continued in a most distressed condition till sun-set; and
 " then expired, in all the anguish which ever attends this most
 " terrible of human disorders.

" He appeared to possess a considerable degree of reason through-
 " out the whole scene—was perfectly able to distinguish and speak to
 " every acquaintance around him—and was apparently sensible
 " that his dissolution was fast approaching.

" A circumstance peculiarly favourable to his friends and at-
 " tendants, and to be remembered by them with gratitude, was
 " this—that he shewed no disposition, at any time, to do the
 " smallest injury or mischief to any person; but was constant in
 " expressing the warmest and most benevolent wishes for the wel-
 " fare of his family, and of his neighbours."

We shall take leave of this subject, for the present, with insert-
 ing an Extract of a Letter from Dr. Mease, of Philadelphia, to Mr.
 Smith, under date of April 3, 1798.

" Probably you may have seen an account, lately re-published
 " in the newspapers, of the use of *vinegar* in the disease produced
 " by the bite of a mad dog. The same account was published in
 " the year 1792; and as I was then engaged in writing my Inaugu-
 " ral Dissertation on that subject, I took pains to ascertain the truth

"of the fact. As the cure by vinegar was said to have been performed in Friuli, in Italy, I wrote to the Rev. Mr. Hall, Chaplain to the English Factory at Leghorn, and requested him to inquire into the circumstance for me; which he very kindly did; and, in a letter dated the 29th of January, 1793, informed me, that 'the physicians of that place, and of Rome, remembered to have seen the account published in a Bologna Gazette, three or four years before; but as it has never made its regular appearance before any medical society in Italy, it does not merit attention.' Count Leonissa is the physician who is reported to have cured the patient by large draughts of vinegar; and I think it probable, if he had been actually successful, he would not have omitted to publish the case in some of the European journals; especially as he could not have been ignorant of the want of system that exists upon the subject of rabies, and that no remedy, uniformly or even partially successful, had before been discovered.—As the disease has unfortunately appeared, of late, in several instances to the eastward, and as many physicians, and others, may be induced to trust to the vinegar, to the neglect of other more powerful remedies, probably this refutation of the reputed success of that acid may tend to save a life, if inserted in your *Repository*."

As epidemics among other animals besides men are so rife at present, we shall need no apology for introducing whatever may illustrate their history. One additional fact has been reported to us relative to the *cat-distemper*. In Connecticut, it has only been observed in the large towns; and has no where, as far as we can learn, been seen in country places. This suggests the inquiry—How far has this been the fact in other States? We wish that the medical philosophers of our country may not disdain this species of investigation.

The disease among *cattle*, of which a very interesting account is given in our last number, by the Rev. Mr. Parsons, has occurred in several other parts of the country.

We learn that the *foxes*, in some parts of Massachusetts and New-Hampshire, have been affected with a disorder, the winter past, which rendered them an easy prey to the hunters. Of the particular symptoms, we have not been able to obtain any correct account.

We are assured also, that *geese*, in some of the Eastern States, have been evidently affected in a singular manner. Many have been seen to seize some object with their bills, and adhere to it till they died.

The concurrence of these facts is curious, and deserves the most attentive observation and research.

Hartford, January 1. As it has been unusually healthy during the year past, we have obtained from the sexton, an account of the deaths which have taken place within the first and second societies of Hartford, in A. D. 1797. These two societies comprise all the town except the society of West-Hartford. The whole number of deaths, from the 1st of January, 1797, to the 1st of January, 1798, is 59, viz.

From 60 years and upwards,	13
Between 60 and 30,	10
Between 10 and 30,	9
Under 10,	27
Total,	59

In A. D. 1794, there died in the same limits,	110
In 1795,	89
In 1796,	75
In 1797,	59

New-Haven, January 4, 1798.

Deaths in New-Haven, A. D. 1797.

January	5	July	2
February	6	August	5
March	6	September	1
April	1	October	3
May	6	November	9
June	8	December	6
Total	58		

Males 29. Females 29. Whites 52. Blacks 6. First Society 14. White-Haven 8. Fair-Haven 10. Episcopal Society 14. Of neither of the above Societies 8. Strangers 4.

Under a year	16	Between	50 & 60	5
Between	1 & 5	6	60—70	4
	5—20	0	70—80	9
	20—30	3	80—90	1
	30—40	5	90—100	3
	40—50	6		
Under 1 and above		60—33		
Under 1 and above		70—29		
Between 1 and		60—25		
Between 1 and		70—29		

A common mortality for the city of New-Haven in a year is 60, or one to seventy souls.

Two fifths of the deaths are under five years; two fifths between five and sixty; and one fifth upwards of sixty. The year last past, the number of deaths among aged people, has been above the usual proportion.

The number of our citizens who die abroad is about the same as that of strangers buried in the city.

Albany, March 2, 1798. At a meeting of the Society for the Promotion of Agriculture, Arts, and Manufactures, of the State of New-York, the following gentlemen were elected officers for the ensuing year, viz.

Robert R. Livingston, President.

Ezra L'Hommedieu, Vice-President.

John Taylor, Treasurer.

Samuel L. Mitchill and Benjamin De Witt, Secretaries.

Robert R. Livingston, Simeon De Witt, Samuel L. Mitchill, Stephen Lush, and William S. Johnson, Committee of Publication.

The annual Address, at the commencement of the Session, was delivered by Dr. Mitchill.

A number of respectable gentlemen, from different parts of the State, have since been elected members; and several important communications have already been received, and read before the Society—among which are Sir John Sinclair's Address to the Board of Agriculture of Great-Britain, on the 20th of June, 1797; and a description of the remarkable *salt springs*, in the western part of our State, with an analysis of the waters, and an account of the salt manufactories at present established, by Benjamin De Witt, M. D. of Albany.

To the Citizens of the United States.

In consideration of the general utility that would result from the citizens of the United States being enabled to procure, free from expence, an analysis of any ores or mineral substances, "The Chemical Society of Philadelphia," on the 20th of June, 1797, passed the following resolution:

"Resolved,

"That a committee of five members be appointed, whose business it shall be to notify, in the different papers of the United States, and by circular letters, that they will give an analysis of all minerals which may be sent them."

In conformity to the above resolution we hereby give notice, that we will analyze any mineral which may be sent to us, provided it be forwarded free of expence, and accompanied with an account of the place and situation in which it was found.

COMMITTEE.

Thomas Smith, No. 19, North Fifth-street.

James Woodhouse, 13, Cherry-street.

Samuel Cooper, 178, South Front-street.

Adam Seybert, 191, North Second-street.

John C. Otto, 37, North Fourth-street.

Professor BARTON of Philadelphia, who has lately published a small tract on the Vegetable Materia Medica of our country, is preparing for the press, a work to be intituled "Strictures on the Arrangement of the Materia Medica, adopted by Dr. Darwin in his Zoonomia."

Dr. Currie of Philadelphia, whose work on Bilious Fevers, announced in our last number, has since made its appearance, promises a work of considerable extent on the *Yellow Fever*.

January 19, 1798. The *American Philosophical Society* held their annual election of officers on the first Friday of this inst. when the following were duly chosen.

President—the Hon. Thomas Jefferson.

Vice-Presidents—Nicholas Collin, D. D. Dr. Benjamin Rush, and Dr. Caspar Wistar.

Treasurer—Mr. John Vaughan.

Secretaries—Samuel Magaw, D. D. Dr. Adam Seybert, Dr. I. C. James, and Mr. Samuel H. Smith.

Curators—Mr. Charles W. Peale, Dr. Benjamin S. Barton, and Mr. Robert Patterson.

Class of Counsellors for three years—Mr. Jonathan B. Smith, Dr. William Currie, William Smith, D. D. and Mr. Jonathan Williams, two years from January, 1798.

The Lancaster German Paper mentions, that on Tuesday, the 11th of January, 1798, between the hours of two and three in the morning, there was felt, in that town and its neighbourhood, a severe shock of an EARTHQUAKE, which continued for several minutes. The shock was accompanied, it is said, with a blaze not unlike the burning of a chimney. To this imperfect account, we have been unable to obtain any additional information.

Dr. Davidge, of Baltimore, has lately published a small work on the Yellow Fever; and we are informed that Dr. Potter, of that city, designs soon to publish a reply to it. We anticipate much pleasure and improvement from the extensive discussions on this important subject, which are now fast spreading through every part of our country.

The following Article is from a Letter, just received, from Dr. Brickell, of Savannah, whose Botanical Communications we shall always be happy to admit.

"I here present you with a description of the *JEFFERSONIA*, one among the many anonymous plants which I have found in Georgia; and which I have named in compliment to Thomas Jefferson, our Vice-President, whose great political and philosophical talents reflect very high honour on the United States; and who, to his immense stores of other knowledge, has added the science of Botany.

"*JEFFERSONIA*. Pentandria, Monogynia. The calyx below, five short oval imbricated leaves; corolla monophyllous, funnel-shaped, on the receptacle, subpentangular, bearing the filaments near its base, its margin hypocrateriform, divided into five round dents nearly equal; style filiform, shorter than the petal, but longer than the stamens; the stigma quadrifid; anthers erect, linear, sagittated; fruit two univalved, carinated, polyspermous capsules, coalesced at base, opening on their tops and contiguous sides, having flat seeds with a marginal wing.

"I have observed one beautiful species, of this genus, which I have named *Jeffersonia sempervivens*, Evergreen Jeffersonia. It is a shrub with round polished twining stems, which climb up on bushes and small trees; the petioles short, opposite; leaves oblong, narrow, entire, evergreen, acute; flowers axillary, yellow, having a sweet odour. The woods are full of this delightful shrub, which is covered with blossoms for many months in the year."

FOREIGN.

Paris, 25 Vendemiaire, October 16.

ASTRONOMY.—The comet which I discovered the 27th Thermidor, at 10 o'clock, P. M. at the National Observatory, has been seen the 28th at Padua, by M. Taoldo's nephew, and at Leipzick by M. Rudiger; at Viviers, on the 29th, by Citizen Flaugergue; at Vienna, in Austria, by M. Trattinick; and at London by Mr. Walker. It has been also seen at Berlin by M. Bode; at Bremen by Dr. Olbers; at Bern by M. Tralles; at Marseilles by Citizen Blaupain; Citizen Mechain observed it, for several days, near Rhodes.

The comet having passed near the earth, I took care to correct the observations of the parallax.—The elements are calculated according to the analytic method of Citizen Laplace. From my own

observations, and from those of Citizen Messier, I have drawn the following elements:

The instant of the passage of the comet through the *perihelion*, the 21st Messidor, at 2h. 53m. 52s. mean middle time at the Observatory.

Distance perihel.	- - - - -	0,52545
Hodus. Ascendeus.	- - -	329° 16 3511
Perihel. on the orbit	- - -	49 34 44
Inclinat. of the orbit.	- - -	50 35 50

The sense of the motion retrograde.

BOUVARD,

Astronomer of the National Observatory.

[*Courier de l'Europe.*]

Paris, 3 Brumaire, October 24. The following article has appeared in the Journal de Paris.

"Citizen Beauchamp, who arrived at Trelifond on the 26th of June, 1797, returned to Constantinople on the 4th of September. He writes to Lalande, that he surveyed the chief points of the Black Sea, which the ignorance of the Turks and the jealousy of the Russians had hitherto covered with a veil. He has found the latitude of Sinope to be 42 deg. 2 min. instead of 41 deg. as it was set down in the best charts: so that the breadth of the Black Sea, believed to be 62 leagues, is only 37. So considerable an error deserved to be rectified by so zealous an astronomer. Citizen Beauchamp bestows the greatest encomiums on his pupil, Charles Hyacinthe Receveur, who, at the age of 18, calculates and observes in a surprising manner. They were to have set out, on the 20th of October, for Bagdat, from whence they are to go to Mascate, in Arabia, where Citizen Beauchamp has been appointed Consul."

The following Articles of Medical Information are extracted or abridged from the 2d volume of the "Annals of Medicine," which was not received in season for a more copious display of particulars.

A small book, in the Italian language, was published at Vienna in 1797, by Count Berchtold, giving an account of a successful method of cure, in the *plague*, which has been discovered by George Baldwin, Esq. his Britannic Majesty's Agent and Consul-General at Alexandria in Egypt. Mr. Baldwin communicated his plan to the Rev. Lewis de Pavia, Chaplain and Agent to the Hospital called St. Anthony's, at Smyrna; who, after five years' experience, pronounces it to be the most efficacious remedy hitherto made use of for the space of twenty-seven years, that the Hospital has been under his management.

The directions are simply these.—Immediately after a person is

perceived to be infected with the plague, he must be taken into a close room, and over a brazier of hot coals, with a clean sponge dipt in warm olive-oil, his body must be very briskly rubbed all over, for the purpose of producing a profuse sweat. During the friction, sugar and juniper berries must be burnt in the fire, which raise a dense and hot smoke that contributes to the effect.

The friction ought not to be continued more than four minutes, and a pint of oil is enough to be used at each time.

In general, the first rubbing is followed by a very copious perspiration, but should it fail of this effect, the operation may be repeated, first wiping the body with a warm dry cloth; and in order still further to promote perspiration, the patient may take any warm sudorific drink, such as elder-flower tea, &c.

It is not necessary to touch the eyes; and any other tender parts of the body may be rubbed more gently. Every possible precaution must be made use of, to prevent the patient from taking cold, such as keeping covered those parts of the body not directly under the operation; nor must the linen be changed, until the perspiration has entirely subsided. The operation should be repeated once a-day, till evident symptoms of recovery begin to appear.

If there are already tumors on the body, they should be gently and more frequently rubbed, till they appear to be in a state of suppuration, when they may be dressed with the usual plasters.

The operation ought to be begun on the first appearance of the symptoms of the disease; if neglected till the nerves and the mass of blood are affected, or a diarrhœa has commenced, little hopes can be entertained of a cure; but still the patient should not be despaired of, as, by an assiduous application of the means proposed, some few have recovered even after diarrhœa had commenced.

During the first four or five days, the patient must observe a very abstemious diet; the author allows only a small quantity of vermicelli, simply boiled in water. Nor must any thing be taken for the space of thirty or forty days, except very light food; as, he says, an indigestion in any stage of the disorder might be extremely dangerous. He does not allow the use of wine till the expiration of forty days.

There is no instance of a person rubbing the patient having taken the infection. He should previously anoint himself all over with oil, and must avoid receiving the breath of the infected person into his mouth and nostrils. The prevention to be used, in all circumstances, is that of carefully anointing the body, and living upon light and easy digestible food.

One of the most ingenious observations made by Mr. Baldwin, is, that among upwards of a million of inhabitants carried off by the plague, in Upper and Lower Egypt, during the space of four years, he could not discover a single oil-man, or dealer in oil.

Mr. James M'Gregor, surgeon to the 88th regiment, corroborates the statement given by Dr. J. C. Smyth, of the efficacy of the nitrous acid fumigations, in arresting the progress of fevers usually denominated contagious—such as, the typhus, &c. The fumigations were extended not only to the Hospital, the clothes and bedding of the sick, but to the barrack-rooms; and the usual means of removal, separation, &c. were employed. The following statement exhibits the progress of the disease, under this new treatment.

From the 17th to the 28th of July, 20 cases appeared; from the 29th to August 4th, 16 cases; from the 5th to the 11th, 10 cases; from the 12th to the 18th, 8 cases; from the 19th to the 25th, 3 cases; from the 26th to the 1st of September, 2 cases; from the 2d to the 8th, 4 cases; and from the 9th to the 15th, one case appeared.

Out of sixty-six patients, only the first died; and though the disease re-appeared about the 20th of October, it was soon vanquished by the same means.

Mr. William Simmons, surgeon at Manchester, in a letter to Dr. Duncan, recommends the use of arsenic—prepared in the manner directed by Dr. Fowler, (in the cure of intermittents) and with similar precautions, for the cure of the hooping-cough—on the credit of three years experience of its salutary effect.

Dr. J. Harness, physician to the British fleet stationed in the Mediterranean in the autumn of 1796, recommends, in very strong terms, the use of the *gastric fluid* of the graminivorous animals, in the cure of very bad ulcers—such as those in scurvy, gangrenous sores in typhus, &c. &c. and adds, that he has known it succeed in more than a hundred instances where sphacelus had occurred.

Mr. Stephen Hammick, jun. one of the Assistant Surgeons to the Royal Hospital at Plymouth, in a Letter to Dr. Garthshore, Physician, London, gives the following Account of the Benefit obtained from the external Use of Hops, in the Cure of large sordid Ulcers.

“ Having obtained permission from Dr. Geach, senior surgeon
 “ of this hospital, whose kindness and excellent advice I experience
 “ on every occasion, to make trial of common hops in those wards
 “ which I attend as his assistant, I have now the satisfaction of de-
 “ claring, that, during the last six months, I have seen very good
 “ effects from hops, in poultices and fomentations, applied to ul-
 “ cers of the worst kind, in more than sixty patients, received
 “ into this hospital from the ships of war. Some of the ulcers
 “ proceeded from scurvy, and some from other causes. But
 “ though all of them have been sordid, fætid, and extensive, yet

“ the scætor has soon been corrected by these applications, and the
“ ulcers have ceased to spread.

“ A large handful of hops is to be boiled with a quart of water,
“ till a strong decoction be formed. Oat-meal, with lard or oil,
“ is then to be mixed with the hops and the decoction, till the
“ poultice becomes of a proper consistence. The poultice is then
“ applied to the ulcer, without any intervening lint. But, previ-
“ ously to this application, the ulcers are directed to be well fo-
“ mented with the decoction. The pain proceeding from the
“ ulcers is soon alleviated, and the ulcers themselves soon cease
“ to spread. They become clean, and in a state to be dressed with
“ lint, or any soft ointment.”

*Dr. John Wilson, Physician at Spalding, in Lincolnshire, in a Letter
to Dr. Duncan, gives him the following Account of the good Effects
of the Argentum Nitratum, in Cases of Epilepsy.*

“ After having tried the various means which are commonly re-
“ commended in epilepsy, without producing any good effect, I
“ have lately employed the argentum nitratum, in doses of two
“ grains and a half, three times a day, with the happiest success.
“ I am now giving it to a boy of 16, in that dose, without pro-
“ ducing any other effect on the system than a slight nausea. He
“ has had no return of fits for ten days past, though they formerly
“ recurred two or three times during the day. He took no other
“ medicine whatever, combined with the argentum, as it was
“ formed into pills with bread-crumbs.

“ I have met with some few cases of dropsy, arising from a dis-
“ eased liver, in which I have employed the succus spissatus ci-
“ naræ, with the best effects, after many powerful remedies had
“ failed. It promoted a flow of urine, and induced a gentle
“ perspiration on the body. The swelling of the legs gradually sub-
“ sided, and the abdomen soon after became of its natural size.
“ In two cases where I have employed it, there has not been the
“ slightest return of the disease.”

Dr. Mosman, of Bradford, in Yorkshire, in a letter to Dr. Duncan, gives him an account of a remarkable case, in which epileptic fits were arrested, by extending the jaws, and keeping the teeth asunder.—This expedient was first attempted, with a small piece of wood, to keep the patient (a child) from biting his tongue. The wood was afterwards laid aside, and the jaws kept open by the fingers of an assistant forcibly applied. The fit was instantaneously terminated by this application of force in opening the jaws; and upon frequent recurrence afterwards, was as often terminated in the same manner. The progress of some teeth was supposed to occasion the return of the fits. Scari-

fication of the gum was resorted to, and the fits no more returned.

The following Observations were made by the late Mr. David Mac-liesh, during the Years 1795 and 1796, while he was Surgeon to the 57th Regiment, then in the Island of Corsica.

“ 1. Relapses into paroxysms of tertian intermittent fevers, as far as I have been able to observe them in Corsica, take place, almost universally, at intervals of 8, 14, 21, or 28 days.

“ 2. In cases of obstinate tertian and quartan agues, which had resisted Peruvian bark, and every other medicine that occurred to me, even moderate doses of arsenic, I have used opium in substance, with the constant and uniform success of curing every such obstinate case, one quartan alone excepted. The number in which I have made the experiment is considerable. After the usual evacuation by vomiting and purging, I have begun with doses of three or four grains of pure opium, three hours before the expected paroxysm. For the second dose, I gave five or six grains; and I thus gradually augmented it till I reached a dose of twelve grains, than which I have never given any larger. I have in no case found any bad effects from this practice: I have observed it most speedily successful when the Peruvian bark has failed.

“ 3. I have used the *zincam vitriolatum* in the intermittents of Corsica, in considerable doses, and in a great number of cases, with remarkable success. I have also used crude sal ammoniac with a good deal of success.

“ 4. I have, in forty-two cases of tertian ague, after the failure of Peruvian bark, and other common remedies, used Dr. Fowler's solution of arsenic, by means of which the greatest part of them were cured. The doses were, at an average, fifteen drops three times a day; but sometimes twenty were given. No bad consequences followed this remedy; which I therefore think an useful one in obstinate intermittents.

Dr. Thomas Garnett, Professor of Physic, in Anderson's Institution, Glasgow, in a Letter to Dr. Duncan, gives him the following Account of the Benefit he has observed from the oxygenated Muriate of Pot-ash, employed as a Medicine.

Dr. Garnett had, for several years, supposed, that when a deficiency of oxygen occurs in the system, that principle might be better supplied by means of the oxygenated muriate of pot-ash, than by any other method. The following case gave him opportunity of subjecting his theory to the test of experiment.

“ A lady, aged 33 years, had been for several years affected with symptoms of scurvy. Vegetable acids, the elixir of vitriol,

and the cold bath, had afforded her some imperfect relief.— She was advised to inhale oxygene gas, but that not being done, it was determined to try the effects of the oxygenated muriate of pot-ash. She was directed to take three grains of it, four times a day, in a little water. As this produced no uneasiness, the dose was increased to double that quantity. She soon found the smaller livid spots disappear, and the larger ones become less. The strength became greater than for years; her countenance grew much clearer, and she was able to take a good deal of exercise. Her menstrual discharge, which the disease had rendered very profuse, became more natural in quantity.

“It is well known, that about 75 cubic inches of extremely pure oxygenous gas may be procured from 100 grains of this salt, by means of heat. The oxygene is so loosely attached, that it is disengaged even by the light of the sun. Would not this salt be an useful remedy in typhus?”

Dr. Swediaur, in a late publication, from a variety of arguments, endeavours to render it probable, that mercury, in curing the venereal disease, as a specific and sure remedy for syphilis, acts only on the venereal virus, in virtue of the oxygene which enters the different mercurial preparations and compositions. Among other particulars, Dr. Swediaur mentions, that citizen Alyon, of the military hospital Val de Grace, at Paris, has read to the Society of Medicine, a paper on the anti-venereal and anti-psoric powers of oxygene, which throws much light on this important subject. Mr. Alyon has particularly employed, externally, the super-oxygenated muriate of pot-ash, for the cure of chancres and syphilitic ulcers, and has found the good effects from it, more expeditious and more certain than those of any mercurial preparation.

Dr. Rollo's letter to Dr. Duncan, enclosing the above-mentioned publication of Dr. Swediaur, subjoins, “in the hospital “of Woolwich, we go on with the oxygenated muriate of pot-ash, in lues venerea, and have every reason to be satisfied with “the general results. As yet, we have not detected a single instance of relapse.”

Mr. Bell, author of the System of Surgery, &c. has lately published a new edition of his Treatise on the Gonorrhœa and Lues. In this new edition, Mr. Bell opposes his testimony to that of many other gentlemen, respecting the anti-syphilitic powers of the *nitric acid*. In every case in which he has tried it, it has failed; and he now believes it to be wholly useless. He accounts for the apparent success with which its use has been attended in the hands of others, by supposing that the ulcers, &c. have disappeared

during its exhibition, not from any efficacy in the acid, but from certain causes not very well understood, which, he says, sometimes occasion their disappearance, and even the disappearance of buboes, whether any remedies are administered or not. Mr. Bell thinks the nitric acid, for external and internal purposes, no better than any other acid.

Among the medical papers which appear in this volume, are inserted letters from Mr. George Kellic, a navy surgeon, to his father, a surgeon at Leith, relative to the anti-syphilitic powers of the nitric acid. They contain the histories of five cases, treated with this medicine. The first and second were of *chancres*, and the third of a *bubo*, and were completely cured. The fourth was a constitutional affection of long standing, together with a chancre. The last was cured, but the first resisted both the acid and mercury; and the event of the case does not appear. The fifth case is also incomplete. The acid appears to have had every good effect that could be expected from it.

In this volume of the *Annals of Medicine*, we also find an analysis of the reports on nitrous acid, of which our readers will remember we gave some account in our Number II. and from which we inserted several cases in Number III. The remainder of the information contained in Dr. Beddoes's publication, is not so unequivocally in favour of the acid, as that which we have already communicated. Mr. Baynton reports two cases in which the acid succeeded, after the failure of mercury; one where, under similar circumstances, the patient's health was improved, though the venereal symptoms were not alleviated; and one wherein the acid produced no curative effect. Three cases follow, by Mr. Bowels of Bristol, in which the nitric acid effected no beneficial change. A Letter from Dr. Girdlestone of Yarmouth, informs Dr. Beddoes that the writer had seen no cures produced by the remedy; but that it often increased the disease. Mercurials exhibited in succession, manifested good effects with more than usual rapidity. Dr. Rutherford of Edinburgh, reports that the acid, in his trials, has had various effects: sometimes failing altogether, and sometimes producing the most sudden and happy cures; and that, in some cases where both mercury and the acid had failed, when separately administered, their union has completely obviated the disease. Some additional proofs of the curative powers of this medicine are furnished by Dr. Geach and Mr. Hammick.

The work of Mr. Cruikshank, also mentioned in our Number II. relative to the Nitric Acid, &c. as remedies in Syphilis, is also noticed by the publishers of the *Annals of Medicine*. Mr. Cruik-

shank made trials of the nitric, the oxygenated muriatic, and the citric acids, and the oxygenated muriate of pot-ash. First, four cases are reported of cures made by the nitric acid; these are succeeded by the same number of cases in which the oxygenated muriatic acid was successful in producing cures; three cases follow, with the like event from the use of the citric acid; and the account is concluded with the relation of six cases in which the oxygenated muriate of pot-ash effected cures. The quality in which the four several remedies here tried resemble each other, is in the quantity of loosely-combined oxygene which they contain; and to which Mr. Cruikshank attributes the curative effects. He, however, differs from many others, in referring the cure, not to the neutralization of the syphilitic virus by the oxygene, but to a new action or disease, supposed to be excited in the system by the oxygene, with which the syphilitic action is unable to contend, and by which it is conquered and superseded.

Such is the amount of the information contained in this work, respecting the powers of the new remedies in the treatment of Syphilis; and we can not help thinking, that enough has been done to render it certain that, in many cases, they possess extraordinary efficacy, and merit a decided preference to mercurials. At the same time, many interesting points remain yet to be cleared up; and while any doubt remains, it is much to be wished that the practitioners of the United States, whose situations permit it, would unite with the faculty in foreign countries in this important investigation, and throw into the common stock the treasure of their experience. In this case, we should hope that the now established circulation of the Repository would point it out to them as an eligible medium of public communication; and to this end, we earnestly invite the practitioners in every part of our country to institute trials, and to favour us with the results.

CORRESPONDENCE.

WE have received a lengthy letter from Dr. Felix Pascalis Ouyère—(of whose Medico-Chemical Dissertation, &c. our readers will remember, we gave some account in our No. I.)—expressive of his dissatisfaction with our judgment of his work. The first part of his letter, consisting of counsel respecting the method incumbent on Reviewers, in our situation, to pursue, we are constrained to omit, as unlikely to advance his interests, or the improvement of our subscribers. Such parts as apply more particularly to the *Review* in question, we cheerfully insert, without comment. The public must determine between Dr. Ouyère and us.

“ You request me, first, to consider whether aqueous gas can be formed under the 80th degree of Reaumur (212 of Fahrenheit). I must previously observe, that the denomination of aqueous gas which I make use of, could not but improperly be replaced by that of *steam*, as you call it, which, according to the best dictionaries of your language, signifies the ‘ smoke or vapour of any thing moist and hot’—(Walker’s Dict.). This word, familiar and necessary in any mechanic use or domestic circumstance of life, is by far insufficient to express all the scientific meaning of aqueous gas, which, on the authority of Macquer, Priestley, Hales, Vanhelmont, Fourcroy, Lavoisier, and Chaptal, are the only adapted words to signify any aeriform, expanded and volatile substance, resulting from any kind of fermentation, more or less charged with particles disengaged from the matters it proceeds from.—The aqueous gas, formed by the blood or by the human body, is sometimes very inflammable, and dangerous to life. It has, besides, many other properties, which you are, no doubt, acquainted with. In my dissertation I considered none of its component parts, except the aqueous particles, because, in the animal fluids, the loss of one only of their primitive substances might be sufficient to cause decomposition, and because all the component parts of the blood are not yet exactly ascertained.

“ To answer, now, to your first query, I cannot do better than to mention a little table, which you will find in Fourcroy’s Lectures on Chemistry and Natural History, chap. v. It proves not only that an aqueous gas may be formed under the 80th degree of Reaumur, but that heat from 5 to 10 excites putrefaction, vegetation, and slow evaporation; from 15 to 20, it accelerates putrefaction, vinous fermentation; from 25 to 30, it dries up plants and vegetables of any kind; at 45, it disengages essential oil from corrupted animal substances; and, at 80, it is the second degree of ebullition, in which water must rapidly become an elastic vapour or a gas, &c. The copious and insensible perspiration discovered, and, long ago, ascertained by Sanctorius, the subtle gas thrown out from the lungs in breathing, which our eyes can perceive in certain conditions of the atmosphere, were, Gentlemen, sufficient proofs that the lowest degree of heat in the animal fluids excited the formation of a gas; which will afterwards be copious at the expence of life, according to the increased heat of the atmosphere; and as we have, in

fact, a scale of a thousand affections, experienced by the application of heat on the body, under 80 of Reaumur, your query is quite useless, and has not the character of an objection. The three or four following questions, derived from the first, are, of course, involved in the same predicament. That other of 'the temperature of the vital parts being always the same, at all seasons of the year,' does not give a better induction against the formation of an aqueous gas, by the excess of heat, than it would make us believe that our vital parts, with their temperature, could not be injured by the application of boiling water, of a red-hot iron, or of ice.

"My second dissertation, on the best antimonial preparations for the use of medicine, is also unfavourably mentioned in your Repository. This had no theory to *refute*; you notice, however, 'my more imperfect form of the common accounts of the history and preparations of antimony, than those found in modern books of chemistry.' Such perfect accounts were not wanted by my querists. My task was to compare all the chemical preparations of antimony, both ancient and modern, in their virtual properties: this I performed in five or six pages; and could not, indeed, fix their attention upon any other preparation but the tartar emetic (antimonated tartrate of pot-ash), and the kermes mineral (red sulphurated oxyd of antimony). Your reflection, that to this last I shew 'the partiality very commonly possessed by physicians for a favourite remedy,' supposes, that all that I said about it did not altogether strike you with conviction. You should have given a few words in proof, if possible, to put truth and correctness on your side."

DOCTOR William Currie, of Philadelphia, has addressed a letter to the Editors of the Repository, in which he considers himself improperly treated by some personal observations of Dr. Seaman. (See Med. Rep. No. III. p. 315.) The Editors are sorry to find that a misunderstanding of this sort has arisen; but, at the same time, they are gratified to observe that Dr. Currie, yielding to the suggestion of the inutility and progressive irritations of personal dispute, and the injury to the Repository resulting from its being employed as the vehicle of individual altercation, has consented to wave all reference to personalities, and to rest his vindication on the ground of fact and argument.

As authority for the assertion, that the Yellow Fever, in the year 1795, was introduced into New-York by the brig Zephyr, Dr. Currie remarks, that "A letter from the Health Committee of New-York to Governor Jay, dated the 8th of September, contains such circumstances respecting the brig Zephyr, as rendered it sufficiently probable that the disease was introduced into New-York by that vessel; and a letter from the Inspectors of the Health Office of Philadelphia, to Governor Mifflin, published in the Federal Gazette of September 24th, contains such particular information on the subject from authentic sources, as, in my opinion, reduced it to a certainty."

Dr. Currie cannot adopt the information said to be given by the Physicians of New-York to their fellow-citizens, viz. "*That, without the aid of putrid effluvia they need have no apprehension of a yellow fever spreading among them;*" and proceeds thus—"But my experience and observations convince me that such information cannot be depended on; for, although the contagion by which the yellow fever is propagated, operates with more certainty, in an atmosphere rendered unsalutary by the exhalations of putrefying vegetable and animal substances, it still possesses the power, though in a less degree, of producing the disease where there is no reason to suspect

any prevalence of such effluvia, provided the air be of a certain temperature, and in a confined situation. I had frequent opportunities of seeing this verified last year, in situations entirely excluded from the access of marsh miasmata or other exhalations from putrid substances, in high and dry situations, in small apartments, and in the cabins of ships, from whence the free entrance of the external air was prevented; at a time when floods of rain prevented any exhalations from putrid or putrefying substances from rising into the atmosphere. From these facts it appears that heated air, without the aid of putrid exhalations, especially when calm, is sufficient to render the contagion active and to propagate the disease from one to another."

"That the disease was propagated last year in Philadelphia by specific contagion, which had no connection with the effluvia from putrefying materials, I am warranted in asserting, because the air was preserved cool and free from such effluvia, by the attention which was paid to the removal of all suspicious substances, from whence they could arise, and from the frequent floods of rain, which washed away every impurity, and prevented putrefaction, or, if any putrid effluvia did rise, they were immediately precipitated to the earth. And that the disease was propagated by contagion alone, and not by any change in the constitution of the atmosphere, or any miasmata derived from the situation, soil, or condition of the city, is indirectly proved from the known facts, that all those who avoided intercourse with the infected, their houses, furniture, cloathing, &c. though daily abroad in the open streets, escaped the disease without exception; as well as all the prisoners in the jail and work-house, the pensioners in the alms-house, and the patients in the hospital, from whence all persons, suspected of infection, were carefully excluded."

"The inhabitants also in the west and north-west parts of the city were exempt, though the situation is more level, and the ponds of stagnant water more numerous, than in the part of the city where the disease first appeared, and became most prevalent. These facts are notorious, and can be attested by almost every inhabitant of this city."

"In a pamphlet which I published the beginning of the present year, I have attempted to demonstrate that the yellow fever, which has, at different times, occasioned so much distress and mortality in the United States of America, differs from the remittent or bilious fevers of hot climates, and not only in degree, but in kind."

END OF VOLUME I.

